ENDEAVOUR

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The future of research

At the moment, the pursuit of knowledge for the sake of knowledge—the Hellenic ideal—is at the lowest ebb throughout the world. In many countries there are neither means nor men to conduct it: in some it is frowned upon as not practical politics. In those which lie under the Axis yoke, indigenous research has been suppressed with every effort at thoroughness that organized brutality can command. Colleges, technical institutions, and universities have been closed, their staffs and students persecuted if not murdered. Scientific textbooks have been burnt, scientific journals compelled to cease publication, scientific societies forcibly dissolved. Yet even in such adverse circumstances the spirit of science has not been extinguished. Stirring accounts have come through of Chinese men of science imperturbably pursuing original investigation in dimly lit caverns, in the depths of forests, and in mountainous recesses. Similar quiet determination is being shown by their counterparts in every part of the enslaved or threatened world. We salute these indomitable souls and bid them be of good cheer, for the day will surely come when their sublime ardour will be rewarded.

What of science in Allied and Axis countries? In both camps the main and practically sole activity of scientists is directed towards the solution of immediate problems of offence and defence, of transport, nutrition, health, and of the thousand other factors that go to make up a total war. Research has one object—to increase the efficiency of the war effort.

Since, therefore, the world of science has been driven by *force majeure* into a position in which its orientation is determined wholly by social conditions, the time is opportune to inquire whether this orientation is not in fact the optimum for peace as well as war. There are in Great Britain many influential scientists who would answer such an inquiry with an unqualified and uncompromising affirmative. To them, knowledge is of value only in so far as it is capable of application to the amelioration of human existence; and, on the principle of first things first, they would urge that the full capacity of science be turned to present needs. In the absence of the urgency of war, it is clear that no such uniformity of direction could be achieved without complete state control, which must therefore be regarded as an essential element of future policy.

No one will deny that the justification of science is the service of humanity. The danger lies in any attempt to convert science from a willing collaborator into a regimented slave. There has admittedly been in the past a wastage and overlapping of effort through lack of co-ordination, and, *per contra*, much important scientific knowledge has been acquired by the deliberate focusing of research ability upon individual commercial or industrial problems. But social and political systems change, often with bewildering rapidity, while the corpus of science develops continually and continuously. To try to force that corpus into any predetermined mould is likely to prove fatal to both—though fortunately the vitality of science, as its history has repeatedly demonstrated, is capable of withstanding extremes of rigour.

The ideal to be aimed at in the development of scientific research is not mechanization of the whole army of workers, for there can be no reasonable doubt of the dreary mediocrity that would follow or of the scientific genius that

would fail to flower. Imagine NEWTON, with no leisure for physics or mathematics, compelled to spend his days devising new methods for assaying bullion, FARADAY commanded by GLADSTONE to discover 'something useful,' or EINSTEIN instructed by a government department to check the tables of seven-figure logarithms! That these are not tendentious exaggerations of what might happen is abundantly witnessed by the records of all governments in all countries and at all times. Let us indeed have more co-operation, more pooling of information, more purposeful and economical direction of research, but let us resolutely withstand the deadening fetters of bureaucracy.

Freedom for the scientist to follow his own bent is, in fact, the *sine qua non* of vigorous scientific progress. If an individual scientist adds to our knowledge of natural phenomena, he has justified his existence, even if the facts he discovers are of no apparent use to the community. No knowledge of Nature is inherently useless, and no one can say that the seemingly trivial observation may not become significant. CAVENDISH'S eighteenth-century bubble of argon was a chemical *objet d'art;* today the applications of this gas are so extensive as to necessitate its extraction on an industrial scale. And, happily, there are still those who inquire into the nature of things because 'all knowledge and wonder (which is the seed of knowledge) is an impression of pleasure in itself.'

Correspondence

Contributions and correspondence should be sent to the editor: E. J. Holmyard, M.A., M.Sc., D.Litt., F.I.C., Imperial Chemical Industries, Nobel House, Buckingham Gate, S.W.1. Scientists engaged in research of an interesting or important character are invited to send short notes on work in progress and results obtained.

FACILITIES FOR INDUSTRIAL AND GOVERNMENT SCIENTIFIC WORKERS

From A. V. Hill, O.B.E., M.A., Sc.D., F.R.S., M.P., *Secretary of the Royal Society*

It is essential, for the good of their souls (and their work), that scientific people employed in industrial and Government laboratories should, in ordinary times, enjoy similar facilities as exist in universities and research institutions:

(*a*) for discussions, lectures, colloquia, visits, attendance at meetings of learned societies, etc. (i.e. for making their science part of their social life, and vice versa);

(*b*) for travel, and for exchange (temporary or permanent) with other laboratories at home and overseas;

(*c*) for receiving visiting and (where possible) foreign co-workers; and

(*d*) for publication of results: secrecy is sometimes necessary, mystery never.

A man's work should be judged by his peers. Without proper facilities this is impossible, and disappointment or complacency, with consequent sterility, may result.

As a preliminary to action on these lines after the war, ENDEAVOUR might invite contributors to put on record now what the facts are as they have found them.

CO-OPERATION BETWEEN UNIVERSITIES AND INDUSTRIAL TECHNICAL STAFF

From R. E. Slade, M.C., D.Sc.

The problem of arranging closer co-operation between academic science and industrial research, discussed in PROFESSOR ANDRADE'S letter [in this Journal, Vol. I, No. 1, p. 7], deserves consideration by those responsible for the direction of industrial research. The views of other academic scientists would also be of interest.

From the start, industrial research has been based upon the use in industry of the outlook and methods of research developed in university laboratories. And industry needs a constant flow of younger men into the laboratory

from the university schools in order to ensure new technique and fresh vision. It is from the universities that industry seeks a newer and broader outlook upon science.

Although it has, in the past, been the opinion of some academic scientists that the university professor should have no relations with industry, the adoption by industry of academic discoveries in recent years has inevitably brought an increasing number of professors into a closer contact with the men actually engaged in production. When this has taken place, I believe a closer understanding of each other's viewpoint has always followed. The chemical industry has achieved a measure of collaboration, but we are conscious that much more may be done without running any risk of turning the professor into an industrial research worker—which would defeat the aim. In our earliest approaches to the academic scientist we sent men to work with a professor for a period up to three years on a subject chosen by him. This enabled our research managers to visit the professor from time to time, and gave opportunities for scientific discussions of mutual interest.

Frequently we ask a professor to allow one of our research workers, or a man chosen by the professor, to work in his laboratory on some subject in which we are interested, allied to that on which the professor is working. In some of our laboratories committees, including university professors, meet monthly to consider the researches in progress. At others a few professors come at regular intervals to spend the day. Any research worker is thus able to discuss with them any problem he likes. This procedure, which seems to work well, is very similar to Professor Andrade's suggestion. It is preferable to discussions taking place in a committee as it allows closer contact between the professor and junior research workers. No doubt, no one system of collaboration will be found perfect, but I am convinced that industry should welcome contact with academic scientists.

THE VELOCITY OF SOUND

From Charles Graves, M.A.

I have been most interested to hear our fighter pilots discuss apparent vagaries in the speed of sound. It seems that they frequently observe phenomena which they believe incompatible with the textbook figure of 1,100 ft./sec. May I suggest that an article on modern measurements of the velocity of sound, of various notes and under various conditions, would be welcome in an early number of ENDEAVOUR?

NATIONAL PARKS

From E. B. Worthington, M.A., Ph.D., *Director of the Freshwater Biological Association, Wray Castle, Ambleside*

The letter from DR JULIAN HUXLEY [Vol. I, No. 1, p. 7] raises problems on which I would like to add a note or two. The establishment of National Parks in the British Empire has been retarded by the reluctance of administrators to take responsibility for locking up great tracts of country for all time against development by human beings. If it can be recognized that some forms of human endeavour, in addition to those purely scientific and educational, are not incompatible with the objects of National Parks, their establishment would clearly be facilitated.

The most obvious financial return comes from the tourist traffic, and it is satisfactory to know that the Kruger National Park was by 1937 practically balancing its annual expenditure from this source. Apart from tourists, however, every acre of land or water cannot help but produce its annual crop, and I suggest that the development of some forms of animal and plant products is not impossible in National Parks.

For example, the Edward Nyanza lies partly in the Parc National Albert, where the killing of all animals, including fish, is forbidden. In part of the lake which lies in Uganda, however, a native fishery has been developed since 1935, and by 1939 produced more than 1,000 tons of dried and salted fish a year, worth £24,000. This fishery if properly controlled is not likely to cause the loss of unique species which inhabit that water, and it would be an asset rather than the reverse in the event of the area in which it lies being declared a National Park.

Coming nearer home, the conversion of an area such as the Lake District into a National Park would not prevent fishing in the lakes or the use of the fells as sheep-grazing. Rather would it stimulate the scientific development of natural resources, often to the advantage of the area.

Physical research on problems of soil cultivation

B. A. KEEN

Dr Keen's article challenges many traditional ideas about agricultural methods. In particular, he brings evidence to show that, contrary to deeply ingrained tradition, crop yields are remarkably insensitive to variations in cultivation; and that the attractive 'capillary tube' theory of the movement of soil moisture is entirely untenable. The work described is likely to have far-reaching effects upon agricultural thought.

When the scientist first seriously turned his attention to agriculture he was confronted with traditions—true, partly true, and false. The beginnings of agricultural science were to a considerable extent concerned with the examination of these traditions, with the separation of truth from falsehood, and with their restatement in scientific terms. Agricultural science is a young growth, for although the literature shows that, almost from the beginnings of scientific inquiry, attention was given to agriculture, such attentions were in the nature of incursions. The present-day agricultural research laboratories, staffed by chemists, biologists, physicists, and even mathematicians, are, with rare exceptions, the product of the past fifty years. The physicist was a late arrival to this team. It is true that a century ago SCHÜBLER, in Germany, worked almost exclusively on the physical properties of soil and that, in Britain, SIR HUMPHRY DAVY'S *Elements of Agricultural Chemistry* balanced his somewhat backward notions of soil chemistry by stressing the importance of soil physics; but before those pioneer works could be followed up the brilliant researches and field experiments of LAWES and GILBERT at Rothamsted, which overthrew the much-advertised ideas of LIEBIG on plant nutrition, had brought soil chemistry and biology to a position where a complete theory of soil fertility seemed within easy grasp. Soil physics fell into the background and remained there until the latter part of the nineteenth century. Its re-emergence was due largely to United States workers: especially HILGARD in California and KING in Wisconsin. In the climatic and soil conditions of America the remarkably swift spread of arable agriculture over that continent had thrown up urgent soil problems. Hilgard was specially concerned with irrigation and soil-alkali problems, and King, working under higher but still insufficient rainfall conditions, made important practical studies on the effects of tillage operations on the moisture content of the soil. In the more settled agricultural conditions of Europe there was a less striking but steady revival of interest in soil physics, marked by the appearance, over the years 1878–98, of a German journal, edited by WOLLNY, devoted entirely to original articles on the physical properties of soils and plants and on agricultural meteorology. At the end of the nineteenth century soil physics had become a recognized branch of agricultural science. WARINGTON, at Rothamsted, summarized the existing knowledge in an excellent little book, *The Physical Properties of Soil*. But the subject was outgrowing its existing theoretical framework. The time was ripe not only for a critical re-examination of the framework by competent physicists, but for its extension to include a study of the colloidal properties shown by all finely divided materials. In Great Britain the subject was reopened at Rothamsted in 1913. Much attention has been given to field and laboratory studies of the laws of moisture-movement in the soil and to their practical bearing on tillage operations and crop growth. Alongside this work, long-range researches on the physical and physico-chemical properties of clay were made, because in the vast majority of soils this finely divided

material has a controlling influence on soil properties and behaviour.

It is instructive for the present purpose, and fascinating in its own right, to trace over the past few centuries the parallel development of improvements in cultivation implements and the growth of ideas on the physical properties of soil. It is appropriate to confine attention to British conditions, for although 'high farming' originated in Flanders, it was quickly adopted in Britain, and under the urge of the Industrial Revolution Britain took the lead in applying mechanical invention to farm implements.

For conditions up to the end of the fifteenth century, the main sources of information are the surviving illuminated manuscripts and tapestries. These show very clumsy ploughs (figure 1), heavy in draught and quite incapable of turning the almost unbroken furrow-slice of the present-day plough. Rough large clods were left and had to be broken down by wooden mallets (figure 2). The importance attached to malleting as a mitigation of inevitably poor ploughing is possibly reflected in the variety of names the mallet enjoyed: maul, mattock, clotting beetle.

In 1523 FITZHERBERT's *Boke of Husbandry* appeared—the first book in the English language on practical agriculture. From it we learn that the art of soil cultivation was well established. Ploughs varied in design from district to district in an endeavour to meet the requirements of the different classes of soil, and the setting of the implement could be varied to give broad or narrow, deep or shallow, furrows. The setting was a formidable operation involving the adjustment of wedges in various slots, and as to the setting of the share itself, Fitzherbert aptly remarks: 'It is harde to make a man to vnderstande it by wrytynge, without he were at the operation therof, to teache the practyue.'

Interspersed in Fitzherbert's technical instructions for adjusting and using implements we find occasional evidence that he was forming ideas on the physical properties of soil. He recognized the crumbling effect, on heavy soil laid up in ridges, of frost and alternations of dry and wet weather. One of his practical directions may be regarded as a simple experiment on soil tilth: one of the earliest on record. To see whether the land is fit for sowing peas and beans the husbandman is told to walk over the ploughed land, 'and if it synge or crye, or make any noyse vnder thy feet, then it is to wete to sowe: and if it make no noyse, and wyll beare thy horses, thanne sowe in the name of God.'

In the seventeenth century there was steady but slow progress in plough design, and BLITH, in his *English Improver Improved* (1652)—which was copiously quoted by writers up to the end of the century—insisted that the draught of ploughs was too great because of lack of care in the construction and setting of the implement. He fully realized that the crux of the matter was to secure a smooth and natural curve over the region where the rear of the ploughshare met the mouldboard. Otherwise the furrow-slice suffered an abrupt change of direction in this angle, which became clogged with soil, thus greatly increasing the draught.

Until the closing decades of the seventeenth century there was little or no progress in the scientific study of soil problems. Thus MARKHAM, a prolific agricultural writer, was content with a purely armchair classification of soils into 'simple' and 'compound.' Simple soils had no mixtures with others of contrary quality and were either loose sands or stiff clays. Simple soils, however, could be 'diverse': black clay, blue clay, 'clay like unto marble,' coloured sands, and 'sands like unto dust.' The compound soils consisted of clays and sands 'equally or indifferently mixed together.' In contrast with this procedure, which is limited only by the ingenuity of its practitioners, it is refreshing to read of HOUGHTON's experiment, published in his *Collection of Letters for the Improvement of Husbandry and Trade* (1681). Houghton was a Fellow of the young, but well-established, Royal Society, some of whose members were also applying the new spirit of scientific inquiry to agricultural problems. The leaven was at work. The account that follows is taken from MORTIMER's *Whole Art of Husbandry* (1706):

> An Experiment of Mr Houghton, to know what quantity of Sand any Earth or Marle is mixed with, which may be of use to try the nature of several sorts of Land by: He took a piece of Clay such as the Brewers stop their Cask with which is commonly a sort of yellow

> Tyle-clay, which weighed 4 Ounces ½ Averdupois, which he dissolved in Water, and poured off the thick into another Bason till all was gone but the Sand; which stuck not together, but lay loose in the Water, and when it was dry would run like Hour-glass-sand, of which he had about the quantity of an Ounce, being of a yellow colour and something glistening, and some little Stones and other foul matter was with it; and when the Clay was settled he poured off the Water, and left the rest to dry in a Pewter-bason, which hung together, only 'twas full of Cracks.
>
> He tryed likewise Fullers-earth which left a thick settlement, which when dry would easily break to powder, but he could find no Sand in it; neither by the Microscope, nor any Grittiness by rubing of it between his Fingers.

In this simple experiment Houghton obtained not only a measure of the proportion of fine and coarse material in his samples, but also observed differences in their physical properties. He records also some entertaining speculations on the relations between soil and its moisture content. He postulated irregular configurations for the soil particles: 'like a pear, big at one end and small at the other,' 'long with two ends,' 'eight cornered, or of several other figures, by which means they cannot run as water and oil that are supposed round.' Hence the indefinitely divisible spherical pieces of water could penetrate between the soil particles, unless these were so shaped as to lie very close together. In this case only the smallest water globules could enter; the layer would be moist but would not drain. Clearly, Houghton was here trying to explain to himself why impervious clay strata may yet be moist.

The eighteenth century produced rapid advances. TULL'S *New Horse-Hoeing Husbandry* (1731) heralded a revolution on the practical side of soil cultivation, while technical improvements in plough design and construction brought the implement to substantially its present-day form. The Rotherham plough (figure 3), named after its Yorkshire factory, had only one half the draught of the common plough. It seems to have been a development of the Dutch plough, and in fact it was known in Scotland as the Dutch or patent plough. The early history of the Rotherham plough is rather complicated and not free from sharp practices connected with the leakage of trade secrets. The mouldboard was made in wood and cased with an iron plate. There is a strong presumption that the mouldboard shape was derived from quite simple geometrical principles and that these formed part of the trade secrets. Whether this was the case or not, towards the end of the century the geometry of the mouldboard was public property. JAMES SMALL, a 'Plough and Cart Wright' of Edinburgh, and that remarkable man THOMAS JEFFERSON—author of the Declaration of Independence and Third President of the United States—both arrived independently at the solution.

Small made many ingenious experiments, using a spring balance to measure plough draught and constructing his mouldboard of soft wood so that places where excessive frictional wear occurred would show up quickly. He assumed that the ideal shape for efficient work and minimum draught would necessarily be the one that had equal frictional wear over its whole surface. He found that this gently and regularly twisting surface could be set out on a simple geometrical construction, and in his *Treatise on Plough and Wheel Carriages*, published in 1784, he gave the method in detail.

Jefferson's solution of the problem is typical of the man. In his *Journal*, preserved in the Library of Congress, there is a comment, dated 1788, on the clumsy ploughs he saw during a tour in Germany. This entry is followed by his views on the correct principles of mouldboard design and a geometrical method of construction. Some five years later he constructed ploughs to his design and used them on his estate.

The basis of these constructions, of which there are many variants, is that the furrow-slice is simultaneously raised, moved bodily to one side, and twisted. Small's method was intended to raise the slice and move it sideways by equal amounts for each unit of forward travel of the plough, and at the same time to give the slice equal increments of twist until, at the rear end of the mouldboard, the under surface of the slice had rotated through 130° and was, therefore, lying at an angle of 50° with the horizontal. The principle of the construction was to mark 130 equal divisions along a block of wood of suitable length, and then, beginning at the front, the wood was cut away until the angle made by the cut surface

GURE 1 – *A mediaeval plough. (Luttrell Psalter, fourteenth century.)*

FIGURE 2 – *Malleting. (Luttrell Psalter, fourteenth century.)*

Reproduced by courtesy of the British Museum.

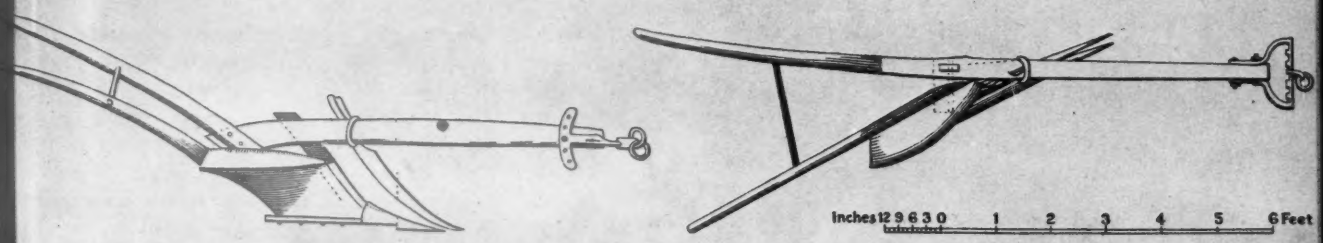


FIGURE 3 – *The Rotherham plough.*

and the bottom at any point was equal to the scale number at that point. The steps were smoothed off, and an iron plate, which formed the actual mouldboard, was shaped to the wooden pattern.

Very little further information on plough design was published after Small's treatise. The nineteenth century produced a bewildering variety of mouldboard patterns, many of which were, beyond doubt, redundant. All these forms, in fact, approximate to a fundamental form which, as BOUSFIELD showed in 1880, can be generated by projection from the successive positions of the furrow-slice itself. Bousfield's method can be followed from figure 4 (*a*), which shows the cross-section of the furrow-slice in the original, vertical, and final positions. In the first 90° it turns around point A, and thereafter around point D_1. Figures 4 (*b*) and 4 (*c*) show respectively the generated plan and side elevation of the mouldboard. The total angle through which the slice is turned is subdivided into equal smaller angles (ten, each of 15°, in this illustration). The length XY of the mouldboard is divided into the same number of equal parts and perpendiculars erected. The outline of the mouldboard is given by the intersections of these perpendiculars with the corresponding lines—drawn parallel to XY—from the ten successive positions of the generating line.

The fundamental form of mouldboard is thus helical only for the first 90° of twist during which it revolves around point A in figure 4 (*a*). If the whole twist were helical, the slice would ultimately have to occupy the dotted position. The remaining twist beyond the vertical is, in effect, accompanied by a sideways displacement of the slice from the dotted position to $A_2B_2C_2D_1$, so that the rear portion of the mouldboard is helicoidal, not helical.

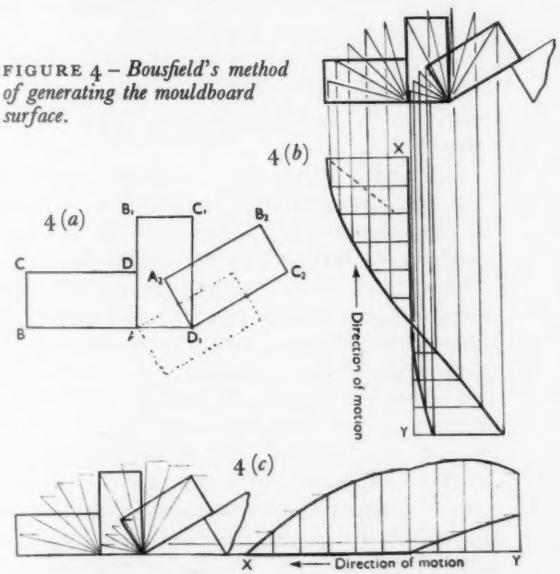


FIGURE 4 – *Bousfield's method of generating the mouldboard surface.*

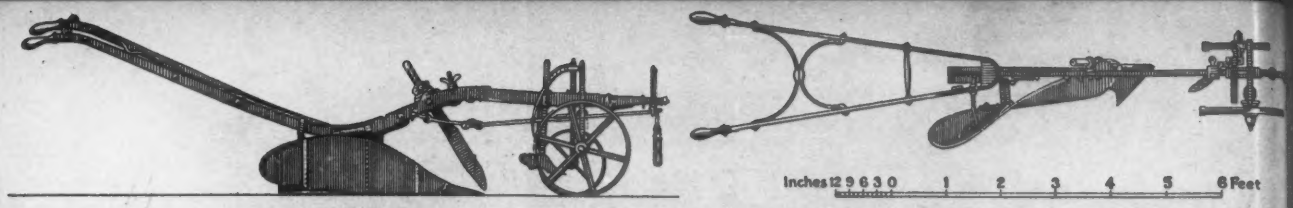
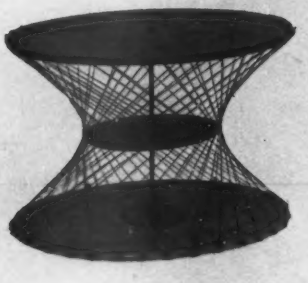
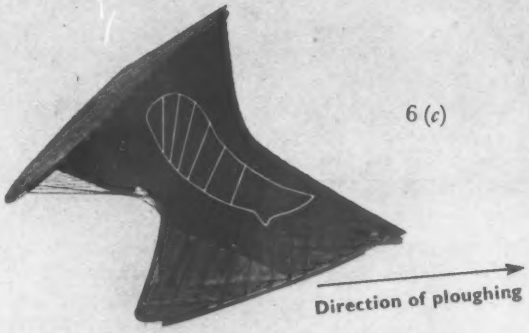


FIGURE 5 – *The modern plough.*

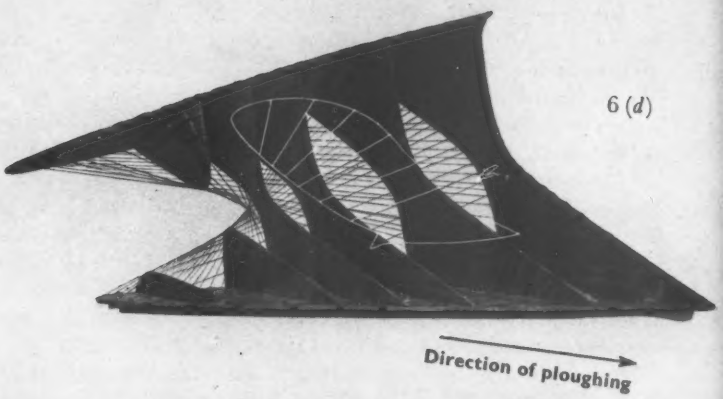


6 (*a*) *Hyperboloid of one sheet.* The surface is generated by the two sets of straight lines shown.



6 (*c*)

6 (*b*)



6 (*d*)

FIGURE 6 – *Models illustrating White's analysis of an American mouldboard.*

This fundamental form is a good approximate representation of the modern British plough. The geometrical construction, however, contains the implicit assumption that a vertical section of the furrow-slice moves in and remains in its original plane or that the soil shears in the same way as a pack of cards. But later work in the United States has shown that there is a twisting of these imaginary planes as the mouldboard moves forward underneath them. Apart from this, there are certain practical requirements necessitating some modification in the derived shape. The cutting edge of the ploughshare lies in the direction of the dotted line (figure 4 (*b*)); this line necessarily lies below the helical surface, so the curvature is altered to merge smoothly into the ploughshare. The construction of figure 4 makes the mouldboard width over its whole length equal to that of the furrow-slice; this is unnecessary, for the slice possesses some cohesion and so the mouldboard width can be reduced, thus saving metal and improving the outline. Over the helical portion of the mouldboard there is a tendency for the slice to be pulled sideways instead of turning on the point A; it has been found that this can be reduced if the generating line is convex towards the bottom of the slice and not straight. Over the helicoidal portion the convexity is progressively reduced to linearity. To press the furrow-slice well home, the tail end of the mouldboard is commonly given a slightly sharper twist.

The approximation of Bousfield's geometrical construction to the form of the modern plough

can be seen by comparing figures 4 and 5. The mouldboard shown in figure 5 aims at turning over the furrow-slice more or less intact and is used mainly in countries where the winter is relatively mild. Frost and the alternations of wet and dry spells produce a progressive disintegration of the furrows, so that in the spring they can easily be worked down into a seed-bed.

In Continental areas where there is a hard winter and the ground is frozen to a considerable depth, autumn ploughing is rarely done. Hence, in the spring, speed of work in ploughing to get a seed-bed quickly is of great importance. The ploughs for this purpose are sharply curved, something like a concave scoop, so that the furrow-slice is partly disintegrated in its passage over the mouldboard. A study of the common types in use in the United States has been made by WHITE, who found that they form part of the surface of a geometrical figure known as a hyperboloid of one sheet, whose equation, with the centre of the figure as origin, is $x^2/a^2 + y^2/b^2 - z^2/c^2 = 1$. It is a vase-shaped surface (figure 6(*a*)), whose actual configuration depends on the values given to the constants a, b, and c in the equation. A property of this figure is that two sets of straight lines lie wholly in the curved surface. By exploring a mouldboard surface with a straight-edge White was able to pick out the two sets of straight lines, and then, by tedious but straightforward arithmetic and analytical geometry to derive the equation for the hyperboloid of whose surface the mouldboard formed a part. A model of the relevant portion of the surface, constructed from White's equation for one plough, is shown in figures 6 (*b*), 6(*c*), and 6(*d*). The outline of the plough and one of the two sets of straight lines generating its surface have been fashioned out of stout wire and whitened, so that their position on the surface of the hyperboloid may be clearly seen.

Although White's analysis gives a close geometrical approximation to the shape of the concave, or 'digger-breast,' type of plough, it cannot, without much further research, offer guidance in the converse problem of synthesis. For this purpose one would have to predict the best values of the constants a, b, and c to produce a plough to meet any given set of practical requirements, and to specify—by giving the equation of the mouldboard outline—whereabouts on the hyperboloid the mouldboard would lie. A further point, which throws some doubt on the practical use of the above analysis, is that the two sets of straight lines, which are the fundamental definition of the mouldboard surface, have no obvious relation with the paths taken by the soil particles in passing over the plough: and the equation of these paths must clearly have some vital relation with the shape of the mouldboard and its efficiency. White recognized this last point and made some practical studies, while NICHOLS, in Alabama, and his associates have conducted extensive work on the behaviour of the furrow-slice in relation to the mouldboard shape. They arrived at a different general formula from White, as all their ploughs could be completely generated by arcs of circles moving along and rotating on, or directly above, the line of travel of the wing-tip.

This disagreement in formulae is not surprising. A short arc of moderate curvature can be fitted to a close arithmetical approximation by an exponential, or any conic section, even though its true equation is none of these; but the algebraic form of the generalized equation for the surface will, of course, depend on which basic equation is selected for development. If in these conditions an exponential, for example, is being sought, it is certain to be found, and found in all good faith, even if only because the discrepancies from a genuine exponential fall within the inevitable errors of experimental measurements.

At present, therefore, it cannot be said that investigators have discovered a generalized equation for mouldboard shape: an equation which, by straightforward and predictable alterations in the numerical values of the constants, would embrace all the various mouldboard shapes needed for different classes of work.

Further, the characteristics of the soil itself must enter into the equation, either explicitly or implicitly, for the response of the furrow-slice to the mouldboard curvature and to speed of ploughing must be affected by the soil plasticity, cohesion, moisture content, clay content, and so on. Nichols has done much pioneer work in the endeavour to bring in these factors, but the complex nature of soil introduces serious difficulties in both definition and measurement of such general properties as

plasticity and cohesion. A further complication is that in soil cultivation we are concerned with soil at low to moderate water contents, whereas true plastic behaviour in the accepted scientific sense is shown only at higher moisture contents when the mix has reached the paste and slurry stages. Because of its importance in providing information on the physical and physico-chemical properties of the clay particle surface, much work on clay and soil pastes has been done at Rothamsted by SCOTT BLAIR and SCHOFIELD. Its discussion lies outside the scope of this article. However, it should be noted that their results and methods have already had important technical applications in such diverse directions as ceramics, oil drilling, synthetic plastics, and the preparation of flour-dough, butter, cheese, and toothpaste.

This work of Scott Blair and Schofield forms part of a wide programme of physical research on soil. Generalizations have grown up in soil physics, often based on simple experimental procedure, the results of which seemed to give a qualitative but plausible grouping of various soils into classes. The need for some such specification was as evident then as now, but it resulted in the appearance of many soil 'constants' and 'equilibrium points': most were empirical, some were spurious, others contained not one, but a complex of factors, and yet others depended on the subjective judgment of the experimenter. Part of the task of the modern soil physicist is to explore this collection and to reject, amend, or replace.

These so-called soil constants and equilibrium points fall into one or other of two classes. In one class, the soil and water are kneaded into a paste, and some property is measured on the sample: it may be the force for rupture of the dry block, the volume shrinkage of the block on drying, the lowest moisture content at which the sample can be rolled out without cracking, the moisture content at which it is just becoming sticky, or the moisture content at which the paste begins to flow when tilted. Measurements of this kind were developed and extensively used by ATTERBERG in Sweden in a system of soil classification, and they are of the kind that would naturally be used by agricultural engineers interested in the behaviour of soil towards cultivation implements of various types and designs.

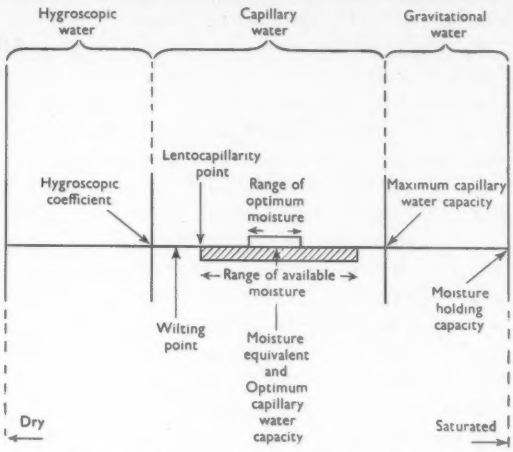


FIGURE 7 – *Diagrammatic distribution of some typical soil-water constants.*

In the second class the soil is used in its natural loose field condition (or in an analogous state of packing), when its pore space will be about 25 to 50 per cent., depending on the type of soil. This class of measurement attempts to specify the behaviour of the soil in natural conditions by a series of moisture contents corresponding to a series of prescribed environmental conditions. It is unnecessary to mention in detail the many measurements proposed and used; typical ones are set out in chart form in figure 7, which gives a general idea of their nature. Some had their origin in attempts to make an agricultural classification of soil types; others were devised in efforts to give some numerical specification of practical observations on soil conditions in relation to plant growth and the effects of cultivation operations.

During the period 1900-25 the development of these soil 'constants,' and the discovery of simple relationships between them, had a fascination for many soil investigators. It was, however, a dangerous occupation, especially for those who did not realize that the ease of making a particular measurement, and the straightforward reproducibility of the numerical result, were no guarantee that the figure expressed any real and simple physical property of the soil. Again, the existence of simple cross-relationships between two or more so-called independent constants was no proof that these constants had a simple physical

interpretation. The primary objection to the incautious development of these constants and cross-relations is that they inevitably lead to classification schemes of the kind shown for soil moisture in figure 7. Superficially, this scheme is in accordance with the field behaviour of soil moisture, but as a foundation for the further development of a basic theory of soil-moisture relationships it is useless. Part of the trouble was that investigators took over from their predecessors without serious question the early theory of soil-moisture movement, based on the 'capillary tube' hypothesis. The theory was simple and attractive, and required no deep physical training for its understanding. Soil was porous, so the pore space could surely be regarded as a bundle of capillary tubes in which water would distribute itself in accordance with the simple concepts of surface tension and capillarity. It was admitted that the tubes must be irregular in cross-section, but it was convenient to assume that that could produce only minor discrepancies.

The essential feature of the phenomenon of capillarity is that the narrower the tube the greater is the height to which water will rise in it. At one stroke, therefore, the observed difference between drought-prone, coarse-grained, or sandy soils and drought-resisting, fine-grained, or clayey ones was explained. In the latter the capillary tubes were much narrower; they could draw up water from the underground water-table through much greater distances than sandy soils could. The traditional belief that hoeing conserved soil moisture had an equally facile explanation: hoeing disrupted the upper inch of the fine capillary tubes and produced a loose mulch with large pore spaces into which the water could not rise and be lost by evaporation. The agricultural operation of rolling young crops had also a simple explanation: the roll compressed the top soil round the young roots so that the narrower capillary tubes thus produced could draw water from the wider ones below for the benefit of the plant. It only remained for some numerical value to be put on the maximum effective height of capillary rise in typical soils and the practical demonstration of the theory would be complete. Laboratory and field experiments, however, showed rises of only 2 to 3 ft., instead of the confident predictions of 10 ft., 30 ft., or more—yet the theory still bore a charmed life.

The first hint that all was not well with it can be found in a paper, *The Dynamics of Soil Moisture*, written by BRIGGS in America as long ago as 1897. But he omitted an apparently unimportant step in developing his argument —a step which over thirty years later was recognized by HAINES at Rothamsted as the key to the problem. In 1917 VERSLUYS in Holland reopened the subject on lines which were correct although obscurely expressed. Later, at Rothamsted, Haines and then Schofield worked out the correct theory of moisture movement in soil and confirmed it experimentally. Haines showed that the pore space in soil was essentially of a cellular nature, consisting of relatively large voids communicating with one another through relatively narrow necks. The moisture distributes itself in curved films within these cells and necks in accordance with the physical principle that it tends to reduce its free surface, and hence its surface energy, to a minimum. The pressure under a curved water meniscus is less than outside, and the greater the curvature the greater is the pressure deficiency. The pressure deficiency, therefore, is a suction force which controls the filling or emptying of the cells.

The manner in which filling or emptying occurs can be seen by taking a simple case. Let a cell and its necks be full of water and suppose evaporation is taking place at one of the necks. The water level in this neck will retreat steadily, until the meniscus reaches the narrowest cross-section of the neck. Here the equilibrium becomes unstable, because further retreat of the meniscus carries it into a wider cross-section where a smaller pressure deficiency than the one already built up would be adequate to maintain equilibrium. Hence expansion into the cell takes place abruptly, or, in other words, air suddenly enters the cell and a portion of the water is displaced to new positions.

Similarly, when the moisture content is increasing from dryness to saturation, once the thickness and curvature of the water film lining the cell and necks attain a certain value, instability sets in. The films in one or more of the necks suddenly close; the stability of the

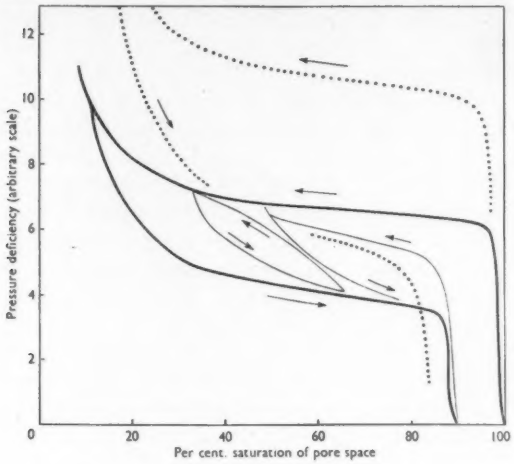


FIGURE 8 – *Haines' moisture content curves based on suction values. (Full curve: 'glistening dew'; dotted curve: sand.)*

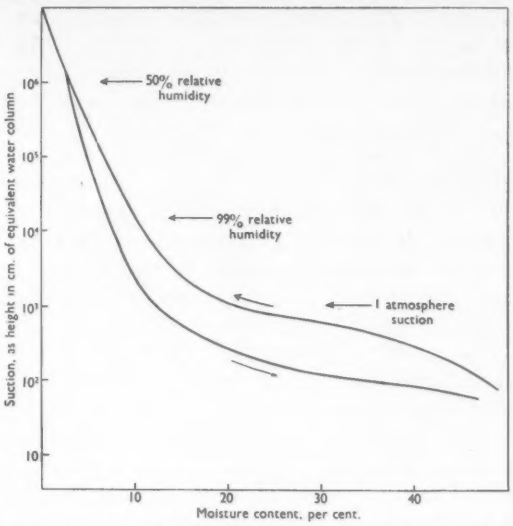


FIGURE 9 – *Schofield's moisture content curves based on energy relationships. (Soil: loam.)*

air bubble in the cell is upset and the cell becomes full of water. One salient feature of the moisture relations is, therefore, the quantum-like movements of water associated with filling and evacuation of the cells. But there is another important consequence of the theory: the opening and the collapse of a neck (and the emptying and filling of a cell) do not occur at the same pressure deficiency. There is no unique relation between pressure deficiency and moisture content. The relationship between them shows a marked hysteresis loop. Hence regions of high and low moisture content can exist in equilibrium together. Haines demonstrated the truth of this by simple and ingenious experiments. The hysteresis loops obtained for a mass of minute glass spheres and for sand are shown in figure 8. The drying curve lies wholly above the wetting curve. Hence the moisture content in equilibrium with a given pressure deficiency is higher if the soil is drying out than if it is being wetted. Again, by suitable cycles of drying and wetting the moisture content can be brought to any value within the hysteresis loop, as shown in the figure.

Haines' treatment of the problem is specially applicable to coarse, sandy soils. Schofield's later contributions produced a generalized physical theory based on energy relationships. Because it deals with energy relationships, this treatment has a great advantage: it is applicable to all soils and holds whatever the mechanism at work. Schofield expresses the differences in suction forces as free energy differences, which can be defined as the height in centimetres of the water column needed to give the suction in question. A logarithmic scale is used because of the wide range of this factor. Thus when the moist soil is in equilibrium with an atmosphere of 50 per cent. relative humidity ('air-dry' soil) the equivalent water column would be about 10 km. high, or higher than Mount Everest. By wetting and drying soils against suction and by using results for soil vapour pressures and freezing point depressions at different moisture contents, Schofield was able to obtain curves like the typical one shown in figure 9. The hysteresis loop is strikingly shown.

An important practical consequence of the hysteresis is that the water in the soil tends to resist changes, whether these are in the direction of increasing or decreasing moisture content. Instead of moving through the pores from regions of high moisture content to low, it will adapt itself to the suction gradient mainly by an alteration in the configurations and curvatures of the water films. To use an expressive Americanism, the water 'stays put'

if it can. The bearing of this on the question of water supply to plant roots is obvious. The capillary theory taught that the drying due to water-imbibition by the roots was met by movement of water from the moister regions, i.e. the water was brought to the plant roots. The correct view, however, is just the opposite: the plant roots have to ramify extensively through the soil in search of moisture. We can now see in their true light the real effects of hoeing and rolling. The action of rolling is primarily the purely mechanical one of compressing loose but moist soil more closely around the roots of the young plant, so that they can more easily obtain the moisture. For any water movement of the type postulated in the capillary theory to take place, the soil would have to be so moist that no farmer would think of ordering the operation: the results on soil tilth would almost certainly be disastrous.

As for hoeing, there can be no general upward movement of water except in those cases of low-lying land where a water-table is very near the surface, and even then the rate of upward movement is exceedingly slow. In this case, as in the much commoner one where the water-table is well below the surface, the tendency of the water to resist changing conditions means, in effect, that it evaporates *in situ:* the dried-out surface layer progressively deepens. Most soils are, in fact, 'self-mulching,' and the use of a hoe to produce a soil mulch is, from the viewpoint of direct water conservation, a work of supererogation, except for those soils that tend to produce a hard crust or cap when drying. In these cases hoeing will break the crust in its incipient stages and so save the tender roots from damage. Of course, by destroying seedling weeds, hoeing prevents their competition for the available moisture and thus, indirectly, the operation conserves water.

The correct idea of soil moisture relationships, outlined above, enables us to form a clear picture of what happens to the rain when it has reached the soil. The soil is, normally, in the condition known as crumbs or compound particles. The crumb is an aggregate of soil particles, especially of the finest ones—the so-called clay fraction—which display this property of aggregation to a marked degree. The crumb itself is permeated by minute pores, much smaller than the pore spaces between

FIGURE 10 – *The root range of sugar-beet.* (*The tape is 5 ft. long.*)

the crumbs. If the soil is fairly dry and rain then falls, it enters the minute pores of the crumbs in the top layer. The excess rain passes downwards through the larger and cell-like pores between the crumbs and, in turn, each layer of crumbs levies toll on it. Depending on its amount and on the initial moisture content of the soil, the fate of the rain will fall between two extremes: it may be held wholly in the upper layers of soil—a frequent occurrence for summer showers—or it may displace water already in the soil, which percolates downwards until it reaches the water-table—a condition which is common in winter, when the pore spaces are practically saturated. Each soil crumb can be regarded as a minute water reservoir which, with any water left behind as a film in the bigger pores between the crumbs, is actively sought out by the exploratory root hairs on the plant's ramifying root system, which traverses its zone of growth so completely that the root hairs are never far away from a supply of moisture. This root system ranges much deeper than is generally thought,

because few subsoils are so tight as to offer a complete barrier to the passage of roots and moisture. Studies of root behaviour made in America, and to a lesser extent elsewhere, show that the roots of common agricultural crops reach a depth of several feet: wheat, oats and sugar-beet, 5 to 6 ft.; barley, 3 to 4 ft.; and potatoes, 3 ft. Figure 10 shows a typical sugar-beet root in a light soil on the Lower Greensand formation. The excavation was 5 ft. deep, and at this depth the main root seen in the picture was still over $\frac{1}{16}$ in. (about 2 mm.) in diameter, and must have extended for at least another foot. Numerous subsidiary and lateral roots were necessarily removed during the digging. The water-holding capacity of the zone of soil traversed by plant roots is considerable. A 5 ft. depth of soil holds at least $7\frac{1}{2}$ in. of rain which, with summer rain, is ample for the transpiration needs of a good crop.

Modern science has shown, therefore, that cultivation operations have only a minor influence on the moisture regime of the soil and, in consequence, much of the traditional belief on this subject must be abandoned or recast. When these traditions are examined closely it is seen that although they embrace the three factors, cultivation, moisture, and plant growth, many are short-circuited into a direct relation between cultivation and plant growth. The gardeners' exhortation to 'keep the hoe moving among the crops' is a familiar example, and it is no exaggeration to say that most farmers believe that their crops will suffer by just the amount that their cultivations fall short of perfection. The belief was instinctive. The art of cultivation was well established long before agricultural science developed, and the practical man naturally felt that the operations were so obviously fundamental as to lie right outside any need for experiment. In settled countries innumerable field experiments have been made on artificial fertilizers and the number of such tests is still increasing, but experiments on cultivation were hardly ever made; yet the costs of cultivation are the heaviest single item in the arable farmer's budget and far exceed those for artificial manuring. In countries where agricultural development came later, experiments on cultivation were made, notably an extensive series in the Great Plains region of America, but the results were ignored in this country for the apparently adequate reason that as they were done in an area of low rainfall, the conclusions could not possibly apply to our own conditions.

Such was the position some dozen years ago, when cultivation experiments were begun at Rothamsted. Slowly but surely—all the more surely because it was against their preconceived ideas—the investigators were driven to the conclusion that crop yields were remarkably insensitive to variations in cultivation. All the experiments were full-scale trials: none can be dismissed on the grounds that it would not apply to practical field conditions. A variety of cultivation procedures was tested on different crops and in different seasons, and the work is still going on. The main results are briefly summarized in the next paragraph.

Subsoiling is a standby of many heavy-land farmers, but on the Rothamsted soil, which has a heavy clay subsoil, the operation produced no effect on sugar-beet. The normal preparation for root crops includes both autumn and spring ploughing, and if bad weather prevents the autumn ploughing some apprehension is felt as to the quality of the spring seed-bed; tests made with potatoes and sugar-beet as the crop showed that the double ploughing gave only an insignificant increase of yield over a single ploughing, and not nearly enough to pay for the cost of the extra work. Deep ploughing every year was no more effective than shallow ploughing, but there was slight evidence that an occasional deep ploughing gave a little increase in the yield of root crops. The nature of the seed-bed itself was also studied and it was found that it had little influence on the final yield. Many experiments have been done on the inter-row cultivation of root crops, in which the minimum hoeings necessary to keep down the worst of the weeds have been compared with a greater number of hoeings. For a number of years the result of the extra hoeings was either no increase in the crop or, more often, a significant reduction in yield, a result due in all probability to the inevitable damage done to shallow-feeding roots. In some of the later experiments on inter-row tillage an increased yield was sometimes obtained. The design of the experiments enabled this positive effect to be traced to one hoeing done in the early stages of plant growth,

and the effect has been confirmed in experiments on another type of soil. It suggests that farm and garden crops are specially sensitive to weed competition in the early stages of their growth and that one hoeing then is worth many afterwards.

The net result of all this work is to show that, provided the crop has a reasonably fair seed-bed, that it is given a hoeing in its early stages, and that the worst of the later weeds are kept down, then any cultivation of the soil—whether before sowing or during growth—in excess of this minimum is of little direct value to the plant. The results are in harmony with those reached by the investigators on the Great Plains area, and they are in harmony with modern knowledge of soil physics. They are in conflict with a deeply ingrained tradition, which had at least three apparently sound reasons behind it, as well as the capillary theory. The first of these reasons was that until the Industrial Revolution provided the farmer with good implements he fought a losing battle against weeds: cultivation at every available opportunity was his only hope. The second reason was that cultivation in the early stages of plant growth sometimes produces striking results, and it was assumed that the yield would benefit correspondingly; the Rothamsted experiments have occasionally shown similar effects which have, however, disappeared by harvest time, so that the ultimate criterion—yield—has not been affected. The third reason was that keen and successful farmers were, almost invariably, enthusiastic exponents of the art of cultivation. This was perhaps the most cogent argument of all to the average farmer: surely these leaders' farms and bank balances proved that their views were correct. But the real reason for their success lay not in the artistry of their cultivations, but in another quality, for which a traditional reason also exists: 'the master's footsteps are the best manure for his land.'

If the present article had been written twenty-five years ago, when the vast majority of cultivation implements was horse-drawn, the results of the cultivation studies now discussed would have had little more than academic interest. A horse costs nearly the same to keep whether he is working or idle, so the real cost of cultivations was negligible. But now, few farms are without a tractor, and the bulk of all cultivations is done with power-drawn implements. A tractor, properly looked after, does not cost anything when idle, and the more it works the higher is the fuel and oil bill. There is, therefore, a powerful economic reason to reinforce the scientific arguments for a complete and critical examination, on all types of soil, of our cultivation methods.

Sir William Bragg, O.M.

It is with deepest regret that we record the death, on 12th March, of SIR WILLIAM BRAGG, O.M., K.B.E., F.R.S., doyen of British physicists, and universally celebrated for his brilliant investigations upon the minute structure of matter. Bragg was born on 2nd July, 1862, and was educated at King William's College, Isle of Man, and Trinity College, Cambridge, where in 1884 he was Third Wrangler. From 1886 to 1908 he held a professorial chair in the University of Adelaide, afterwards becoming Cavendish Professor at Leeds University (1909–15) and Quain Professor of Physics in the University of London (1915–23). In 1904 and following years he showed that the α-rays from different radio-active substances went different distances through different media, and that their range was closely connected with the atomic weights of the atoms in the medium penetrated. He also carried out work on β-rays and on X-rays, and examined the corpuscular properties of the latter. Later, he collaborated in the experiments of his son Sir Lawrence Bragg on crystal structure, and the results of the partnership were of such importance that father and son were jointly awarded the Nobel Prize for Physics in 1915. Sir William also received the Rumford Medal (1916) and Copley Medal (1930) of the Royal Society, and was honoured by universities and learned societies in all parts of the world. His death is a heavy blow to British science, which it deprives at once of a great investigator and a great administrator. One of his last acts was to further with enthusiasm the launching of ENDEAVOUR, to the inaugural number of which he contributed.

The significance of metal single crystals

E. N. da C. ANDRADE

In this article Professor Andrade describes some of the remarkable properties of individual crystals of metals, a subject upon which he himself has carried out fundamental investigations. It may seem a far cry from a single crystal to the foundry, but Professor Andrade shows that some of the most valuable industrial qualities of metals are a function of the qualities of their constituent crystals.

One of the most striking properties of metals, as contrasted with solids of many classes, is that they can, under suitable conditions, be very considerably deformed without rupture, a property denoted by the rather ill-defined term 'plasticity.' On this property are based the industrially fundamental processes of forging, drawing, rolling, extrusion, and such-like, which were carried out for centuries without any systematic inquiry as to the structural interpretation of the changes taking place. In 1864 SORBY initiated the study of metallography, but it was not until 1899 that EWING and ROSENHAIN drew attention to a change which plastic deformation of a metal could produce in the crystallites of which the metal is composed. There appeared in these crystallites systems of parallel lines or ridges, which they attributed to slip along the 'cleavage planes' of the minute crystals. This discovery should have indicated the importance of investigating the behaviour of single crystals of metals under definite conditions of stress, but it was not until 1914 that suitable test pieces, namely thin rods, were more or less accidentally prepared, in which the crystal axes were the same throughout—that is, the rods were metal single crystals in which the external crystallographic faces were not developed, but replaced by an arbitrary cylindrical boundary, just as the external faces of a cut diamond are arbitrary. In the interval between the end of the last war and the beginning of the present war extensive investigations have been carried out on the mechanical properties of such monocrystalline specimens, mainly in England, Russia, and Germany. For some reason the subject does not seem to have attracted American investigators.

Most of the experiments have been carried out by the extension of rods or stout wires in which the single crystal structure has been produced by one method or another of slow growth from an initial crystal 'seed,' although important results have been obtained by G. I. TAYLOR on stouter specimens subject to compression and by CARPENTER and ELAM working with flat specimens. Some of the properties found can be easily reconciled with well-established ideas. For instance, the permanent extension of the crystal specimen takes place by slipping on certain preferred crystalline planes and in a certain preferred crystallographic direction[1]. This seems reasonable, although it can produce queer-looking results when a crystallographically unique glide plane happens to be nearly at right-angles to the axis of the wire, as shown in figure 1, which represents a cadmium wire with the hexagonal basal plane vertical, as the picture is arranged on the page. Tension along the wire has produced considerable slip across the direction of tension, to the left, and a lesser slip on another plane towards the extreme right.

Less easily reconciled with familiar ideas is the fact that, in general, slip does not take place equally on all parallel planes, but more especially on certain arbitrary ones, giving a characteristic appearance of a set of layers, as seen in figures 2 and 3. The spacing of the layers

[1]The direction is more fundamental than the plane and is always atomically the most closely packed direction. See E. N. da C. Andrade, *Proc. Phys. Soc.*, 1940, **52**, 1.

varies from metal to metal, and also with temperature. It may be as small as 1 μ—but even this is about 3,000 crystal plane spacings—or as large as 1 cm. A general rule is that the higher the temperature the wider the spacing. The existence of these 'glide lamellae' points to a lack of ideality in the crystal behaviour, since with a perfect crystal a uniform distribution of glide might be expected. Further, it has been established that if the crystal is left under a suitable load the slips take place in small jumps at discrete intervals of time. Thus the phenomenon is discontinuous in both space and time. We are confronted with small local avalanches.

Other striking general properties of single crystals are their yield under very small forces, and, especially in certain cases, the very large plastic extensions that can be produced without rupture. There are strong theoretical reasons for supposing that the yield point of *perfect* crystals should be much higher than that found for any industrial metal, and the fact that industrial metals do not reach their theoretical limit was sometimes in the past put down to their polycrystalline structure. Actually, however, a metal in single crystal form is much weaker than the same metal as ordinarily found: single crystals show plastic yield under stresses about one hundredth of those given as the tensile strength for the polycrystalline metal. As regards extensibility, single crystals of cadmium, for instance, can be drawn out to as much as five times their original length, the cylinder becoming, on account of the glide in a fixed direction, a flat ribbon. Figure 4 shows this ribbon effect, the marked slip having started to take place, for reasons that would take us too far, on a fresh set of planes where the sudden thinning occurs. Normal to the paper the specimen is equally thick all along.

A further feature of single crystals is the remarkable hardening which they show under large strain, corresponding to the so-called 'work-hardening' of ordinary metals. This is particularly striking when the metal is far from its melting point, and thus, if experiments are done at room temperature, it is strongly shown by metals of comparatively high melting point, e.g. silver, gold, and copper, rather than by lead and zinc. To take an extreme case, it is on record that, with a single crystal of silver, an extension to something over double the original length produced a ninety-two-fold increase of strength.

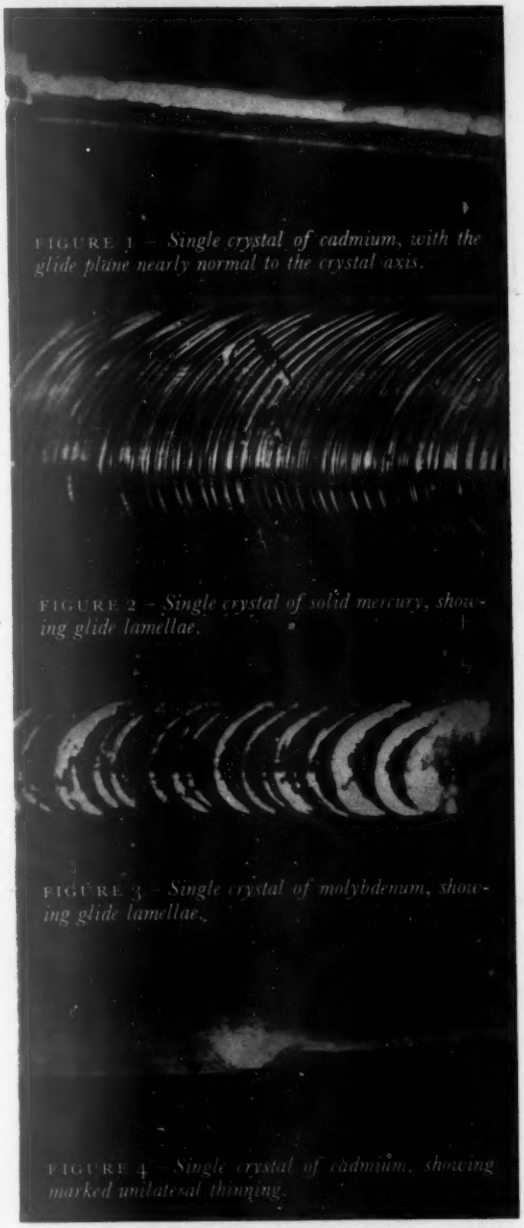


FIGURE 1 — *Single crystal of cadmium, with the glide plane nearly normal to the crystal axis.*

FIGURE 2 — *Single crystal of solid mercury, showing glide lamellae.*

FIGURE 3 — *Single crystal of molybdenum, showing glide lamellae.*

FIGURE 4 — *Single crystal of cadmium, showing marked unilateral thinning.*

We are confronted, then, with the paradox that a single crystal, which might be expected to be very strong, suffers plastic deformation under quite small stresses, but, if the temperature be such that 'crystal recovery' does not

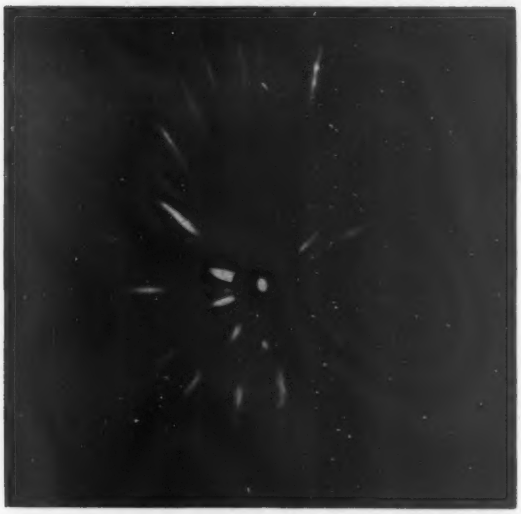


FIGURE 5 – *Laue spots, that should be more or less circular for a perfect crystal, drawn out into asterisms in the case of an iron crystal extended by* 16 *per cent.*

take place, hardens considerably as the deformation proceeds. It can be shown by the use of X-rays that the increasing strength is accompanied by a local break-up of the crystal lattice: the Laue spots, which approximate to small discs in the case of a good crystal, are drawn out into smears or 'asterisms' as shown in figure 5. These general features do not seem capable of explanation on the assumption that single crystals, as prepared, are built on a perfect lattice. Temperature agitation cannot be invoked as the fundamental cause of plasticity because, surprisingly enough, single crystals of cadmium show great plasticity even at only 1° C. above absolute zero.

The only theories that seem to offer even a partial solution of the problem are based upon the supposition that the crystal lattice is not perfect, but contains places of misfit, where the atoms find themselves 'out of step' with their neighbours. It can be shown that at such a spot a shear stress is likely to cause an avalanche-like glide running along the crystal plane. A very imperfect analogy is provided by the way in which a small cut in the edge of a fabric makes it easy to tear, the tear running along a structural line. Thermal agitations may from time to time conspire to cause local conditions particularly favourable to the glide, which accounts for the discontinuity in time of the avalanches. How the places of misfit originate, and whether they are inevitable, is not known. It may be that they are a kind of 'frozen Brownian movement,' and are bound to be formed when a metal solidifies from the melt.

The hardening is closely connected with the considerable disturbances of the structure that the strain itself produces. At higher temperatures these can heal themselves: we have 'crystal recovery' and little hardening. At lower temperatures they are more or less permanent. Probably the courses of the avalanches are stopped by the artificial breaks created in the structure, just as there seems little doubt that they are stopped by the crystal boundaries in a polycrystalline metal, which accounts for its superior strength.

The study of single crystals has brought to light a series of strange phenomena which, it seems clear, originate in the departure of the actual structure from an arrangement on a perfect lattice. At certain more or less regularly distributed spots the regular order is broken: there are 'dislocations,' which may be inherent in crystal structure. These, as potential origins of glide, are the source of the strange softness of an unstrained single crystal, a softness which rough handling remedies, probably by the creation of what are virtually crystal boundaries where the regular structure is broken. The dislocations may be looked upon as plague-spots from which epidemics may be rapidly propagated: at the boundaries there are quarantine stations which prevent the infection spreading. From the point of view of the practical metallurgist we have two tasks: firstly to see if the disease centres can be stamped out or diminished and, if not, to see how the boundaries can be made as frequent as possible and the quarantine as effective as possible. Single crystals in themselves may seem remote from the foundry, but they carry in them the secrets of metallic softness and strength.

The Museum of the History of Science, Oxford

F. SHERWOOD TAYLOR

The development of science resembles the growth of an organism, and properly to understand its present or to estimate its future we need to have as complete a knowledge of its past as is possible to obtain. The paraphernalia of bygone scientists form an integral part of this knowledge. Dr Sherwood Taylor reveals some of the wealth of such historic equipment preserved in the Old Ashmolean building at Oxford.

Science, readier to anticipate the future than to recall the past, has had small regard for relics. Little historic apparatus has survived, and it is well that the Museum of the History of Science should be wholly devoted to these heirlooms of natural philosophy, and should continue to house them in a building which is itself a monument to the scientific zeal of a bygone age. In the revival of science which preceded and followed the formation of the Royal Society, no country took a greater part than England, no place than Oxford. BOYLE, HOOKE, MAYOW, LOWER, WALLIS the mathematician, WILLIS the anatomist, HALLEY the astronomer, all carried out some part of their researches at Oxford, and much of the early *Philosophical Transactions* was printed there. Need was felt for a home of science in Oxford, and the opportunity was made by ELIAS ASHMOLE, fervid if credulous lover of learning. Ashmole had come into possession of the collection of rarities made by the brothers TRADESCANT, and presented these to the University on condition that a home was made for them. So in 1683 the 'large and stately pile of squared stone,' shown in figure 1, was completed. The magnificent portal led into a pillared hall—the School of Natural History; above it was the Musaeum Ashmoleanum; below it a stone-vaulted chemical elaboratory, which today shelters our collections from air attack. There was also a library which once possessed the priceless Ashmolean collection of manuscripts, now in the Bodleian. DR PLOT, author of *The Natural History of Oxfordshire*, became the first curator. Ashmole's high hopes for the institution were not altogether realized. The collections were preserved and augmented, but in eighteenth-century Oxford science sank into a lethargy. Enthusiasts were few and it was then that the museum committed its major scientific crime of destroying the two stuffed dodos, because, forsooth, they had become somewhat dilapidated.

FIGURE 1 – *The Museum of the History of Science.*

With the nineteenth century, science in Oxford revived, but the Ashmolean building proved neither large enough nor adequately equipped for modern science. The laboratory and the scientific collections were removed, and the archaeological collections and the title of Ashmolean Museum were transferred to a building in Beaumont Street. Ashmole's noble building was devoted to housing the scholars who made the great *Oxford Dictionary*. But in 1925 science returned to its home. LEWIS EVANS, learned and

FIGURE 2 – *Astrolabe of F. Egnatius Dantes, in gilt brass. About* 1580.

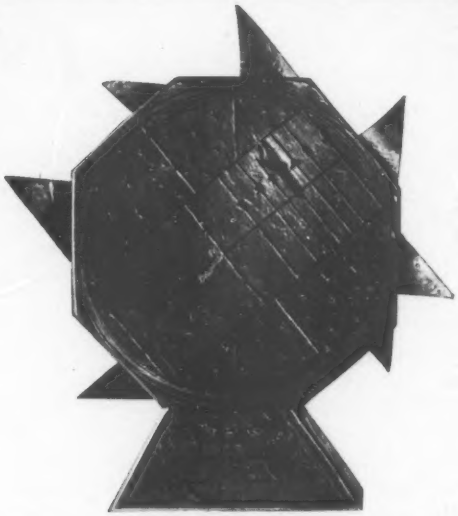


FIGURE 3 – *Block dial made for Cardinal Wolsey by N. Krätzer.*

indefatigable collector of astrolabes, dials and the like, presented his collection to the University, as did Ashmole before him, on condition that it was suitably housed. The collection, augmented by loans and gifts from the colleges and many other sources, was housed in the old Musaeum Ashmoleanum until the outbreak of the present war: after the war, if no evil befall, it is to occupy the whole of this superb building.

The collection ranges over the whole field of science from the Arabs to the twentieth century. Its most remarkable feature is its series of astrolabes. The astrolabe (figure 2) is, briefly, a graduated circle, furnished with sights for taking the altitude of the sun or a star, and a projection of the heavens, by aid of which the time of day and other conclusions can be deduced from the altitude without calculation. Invented by the Greeks, astrolabes were the most important astronomical instruments of the Middle Ages. They were owned not only by philosophers, but by princes, shahs, sultans; no beauty of ornament or skill of craftsmanship was thought too great for them. The same ingenuity and art were lavished upon sundials, and upon nocturnals, which measure time by the wheeling of the Bears about the Pole. The design of dials was a recreation of the geometer, and their ingenuity presents some problems to the modern collector. Our most interesting dial is that made for CARDINAL WOLSEY by KRÄTZER (figure 3), who is thought to be responsible for the Hampton Court clock.

Such men as Boyle and Hooke left few relics. The apparatus of working laboratories rarely survives, for its fate is to be pulled to bits and made into something else. The scientific instruments which achieve the Elysium of a museum case are those bought by rich amateurs, handled a few times, then relegated to a peaceful cupboard for a century or so. Such was the fate of one remarkable collection of instruments which reached our museum. CHARLES BOYLE, fourth Earl of Orrery and great-nephew of Robert Boyle, amassed in the years round 1700-10 a collection of philosophical instruments and bequeathed them to Christ Church, where they seem to have remained almost untouched till 1919. They are now on loan to the museum and preserve for us a collection of the finest work of the instrument-makers of the day, untouched by the fell hand of the renovator who has added later improvements to so many ancient instruments. Out of some fifty items may be mentioned a superb microscope by JOHN MARSHALL (1704), an exquisitely finished model of the Copernican sphere, and several telescopes.

The earliest astronomical telescopes were of wood, while terrestrial ones were usually of vellum, for until the eighteenth century was

well under way it was not possible to make brass tubes to slide sweetly one within the other. The vellum telescopes are very beautiful, their outer cases tooled in gold and stained in soft reds and greens. They are also very light but, of course, lack rigidity. Oxford's observatories, the Savilian and the Radcliffe, have left us many noble instruments, and very notably the great eight-foot quadrant of JOHN BIRD, the high-water mark of eighteenth-century instrument-making.

The museum is rich in orreries—mechanical models of the solar system, which were invented by the instrument-maker ROWLEY and named after his patron the fourth Earl of Orrery. We have not the original orrery, but we have on loan three which are of the first interest both as instruments and as objects of art. The grand orrery from All Souls College (figure 4) not only shows the motions of sun, moon, and planets, but also those of the numerous satellites of Jupiter and Saturn. Microscopes progressed greatly in the eighteenth century, but space allows me to single out but one of our collection, which presents the eighteenth-century idea of a king's microscope. The collaboration of silversmith and instrument-maker produced the magnificent instrument shown in figure 5, absurd but a type of a bygone age.

FIGURE 4 – *Grand orrery by Heath and Wing; lent by All Souls College, Oxford.*

Old chemical apparatus is very rare, for glassware continues in use till it is broken. Yet in the museum is an appreciable part of the apparatus which was used in the old public elaboratory a century and more ago. Glass alembics differing not at all from those used in the Middle Ages, and a collection of miscellaneous apparatus, eudiometers, U-tubes, combustion tubes, a charcoal tube-furnace and so on, form a remarkable exhibit. A very large burning-glass on an elegant stand reminds us that the sun provided the only heat greater than that of a wind furnace. Of chemicals, we possess three cabinets of materia medica, containing many hundreds of named specimens of eighteenth-century drugs, including such items as the skeletons of vipers and black pitchy-looking mumia–dried human flesh. But perhaps the most significant of our chemical exhibits is the germ of a vast industry in the form of an elegant lignum vitae cabinet, lined with red velvet, containing a dozen or so of handsome bottles. A silver plate proclaims that it contains: 'Products obtained from coal according to the PROCESS of Mr. Winsor, prepared by T. Accum, Lecturer on CHEMISTRY in the Surrey Institution.' ACCUM was at this institution between 1803 and 1819. The bottles contain muriate of ammonia, carbonate of ammonia, and a number of products derived from coal-tar. One of these, 'Highly Rectified Essential Oil,' can be nothing else than benzene, prepared a good many years before FARADAY's discovery of it in 1825!

Air-pumps, electrical machines, surgical instruments, barometers, thermometers, dumbly demand description which space unfortunately forbids.

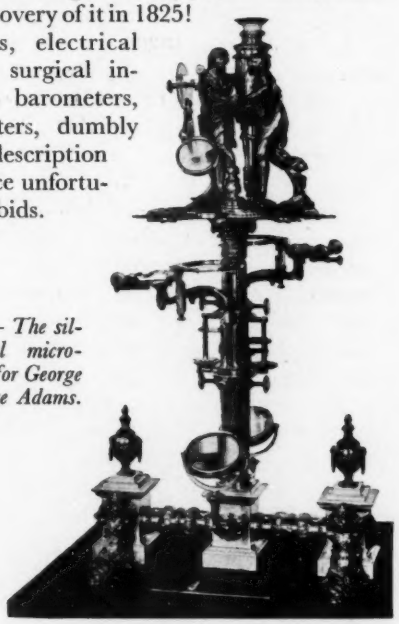


FIGURE 5 – *The silver universal microscope, made for George III by George Adams.*

Textile fibres under the X-rays

W. T. ASTBURY

Not so long ago, elucidation of the structure of textile fibres would have seemed an unrealizable scientific ambition. Dr Astbury is able to demonstrate not only that the problem is by no means insoluble but that, in fact, it has already been largely solved. His original X-ray photographs, with their explanatory text, indicate the line of approach—and explain, incidentally, the scientific basis of the 'permanent wave.'

X-ray structure analysis is a wide subject, but it is still rather a specialist's subject. A grasp of its principles, nevertheless, and an appreciation of its many applications are within reach of all who are trying to advance or apply physico-chemical and biological knowledge. Some of the most fruitful developments in recent years have been in the field of textile and other biological fibres. Natural fibres are found to be constructed very much as textile yarns are constructed from the fibres themselves, only Nature uses long chain-molecules instead. These run along the length of the fibre, or spiral-wise round it, in thin bundles that are in effect submicroscopic crystallites of the fibre substance. The size, shape, orientation in the body of the fibre, and ultimately the intimate molecular structure of the invisible crystallites, are all susceptible of study by X-ray methods, as will be seen best from the examples illustrated and briefly discussed below.

The experimental technique of X-ray fibre analysis is again relatively simple in principle —it is the correct interpretation that calls for the nicest co-ordination of mathematical, physical, chemical, and biological data—and the gist of it will be clear from figure 1. The X-rays are directed in an approximately parallel beam, say ½ mm. thick or less, by means of a cylindrical channel, or 'slit' as it is loosely called, down the centre of a heavy-metal tube some centimetres long, or by means of pinholes in two lead plates. Immediately in front of the slit, and generally perpendicular to the X-ray

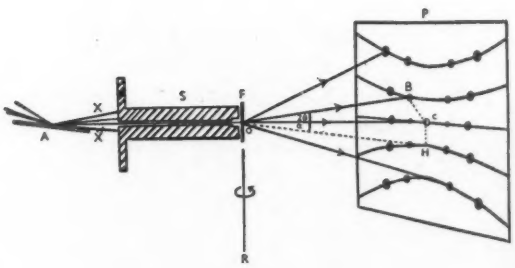


FIGURE 1 – *Illustrating the diffraction of X-rays by a rotating crystal or a bundle of parallel fibres.* A, *anticathode of X-ray tube;* X, *X-rays;* S, *'slit';* F, *fibre bundle;* OC, *incident undiffracted beam;* OB, *a diffracted beam;* P, *photographic film;* C, *lead cup and centre of photograph.*

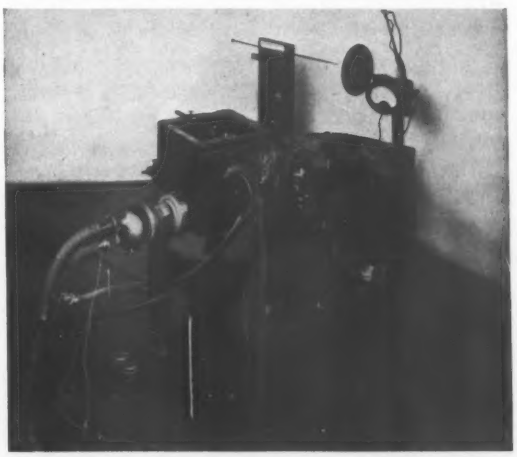


FIGURE 2 – *An X-ray tube with an X-ray spectrometer set up at each window.*

beam, is mounted a small parallel bundle of the fibres under examination, while a few centimetres beyond the fibres is mounted the photographic plate or film, suitably protected from light by black paper or thin aluminium foil (which the rays penetrate fairly easily). The fibre crystallites 'diffract' the incident beam in directions depending on their form, mutual arrangement and molecular structure, and the diffracted beams give rise, on developing the photograph in the usual way, to a pattern of *black* 'spots' or arcs distributed, in the ideal case, along a series of co-axial hyperbolae. Figure 2 is a view of two actual X-ray spectrometers (of different types) set up one at each window of an X-ray tube.

X-rays are short-wave light for the eyes of the mind, so it is important for the lay reader to realize that the pictures shown here do not represent anything that can be seen by the human eye, even with the aid of the microscope: they are 'diffraction patterns' arising from the way the atoms and molecules are arranged. The shape and dimensions of the fibres themselves mean almost nothing: we are looking *inside* where the essential physical and chemical properties reside.

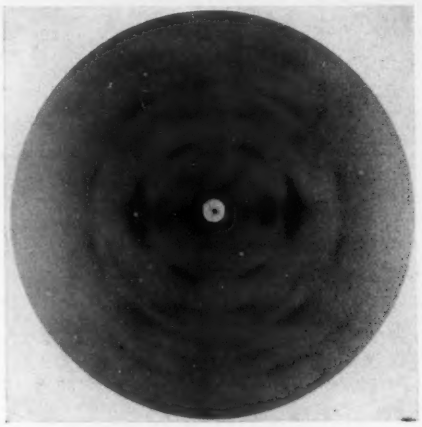


FIGURE 3

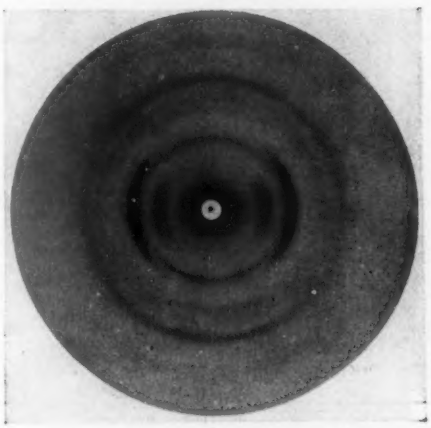


FIGURE 4

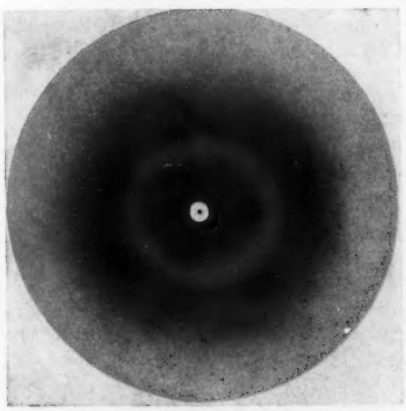


FIGURE 5

Figure 3 – The natural fibre *ramie*, made from the polysaccharide cellulose, the molecules of which are long chains of β-glucose 'residues.' From photographs such as this it has been possible to determine the form of the cellulose molecule almost completely. Notice the broad 'spots' on or near the equator of the photograph as compared with the sharper spots on or near the meridian, which means that the crystallites are long and thin—regular chain-bundles, in fact. A similar effect will be seen in most of the other photographs and for a similar reason.

Figure 4 – *Cotton*, also built from cellulose chain-molecules, but this time laid down in pronounced spirals round the fibre axis, as is shown by the spots being drawn out into arcs.

Figures 5, 6, 7, 11 (*a*) *and* 11 (*b*) – All the wools and other mammalian hairs, nails, horn, hooves, spines, whalebone, etc., give, when unstretched, the pattern shown in figure 5, which is that of the protein *α-keratin*. When stretched they give the pattern shown in figure 6 (*β-keratin*), but when they are allowed to contract again to their original length the α-pattern returns. X-rays thus reveal a reversible transformation of the keratin molecules themselves and explain in this way the beautiful long-range elastic properties of hair, as also those of muscle, the chief protein of which, *myosin*, gives

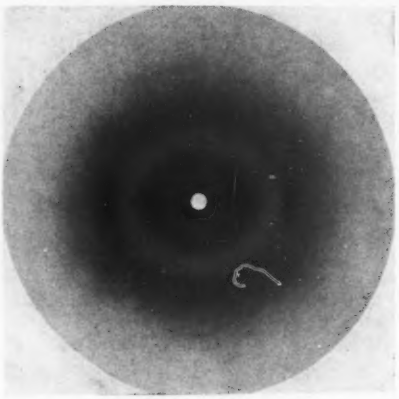


FIGURE 6

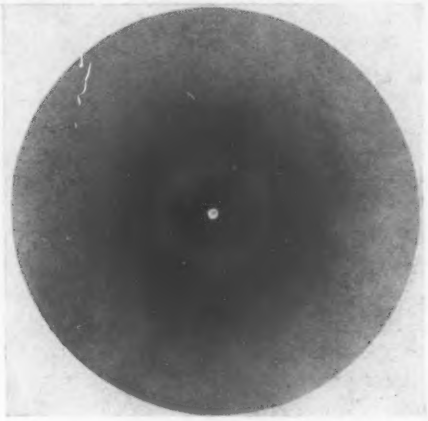


FIGURE 7

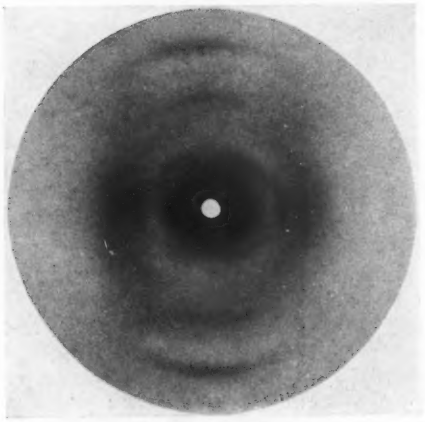


FIGURE 8

X-ray patterns of the same type. The α-form of keratin and myosin is built from regularly folded polypeptide chains which straighten out when the specimen is pulled, then return to their original folded form when it is released. By a development of this interpretation it is possible to explain also the 'setting' of hairs stretched or deformed in hot water or steam, and hence the 'permanent wave'! Figure 7 is the X-ray pattern given by human hair stretched in steam. Figs. 11 (*a*) and 11 (*b*) are X-ray micro-photographs of a single hair, unstretched (*a*) and stretched (*b*). They are the same as figures 5 and 6, only on a much smaller scale because of the special technique involved. A recent case in which X-ray single fibre photographs found a noteworthy application was an investigation of the very rare abnormalities of human hair known as *Monilethrix* and *Pili torti*.

Figure 8 – A parallel bundle of fibres from untanned cow-hide (*collagen*). The collagen fibres include connective tissue, tendons, fish skin and fins, cartilage, gelatin, etc. Leather consists almost entirely of disoriented collagen fibres. Common to the whole group is a characteristic diffraction pattern which has been interpreted as arising from polypeptide chains in a partial *cis*-configuration, as opposed to the slightly longer *trans*-configuration in silk and stretched keratin. Changes in the collagen diffraction pattern give valuable information in the study of tanning and similar phenomena.

Figure 9 – Natural silk (*Bombyx mori*). X-rays show that this fibre (the protein *fibroin*) is built from fully extended polypeptide chains. Like cellulose, it has therefore no long-range elasticity of the type shown by hair and muscle.

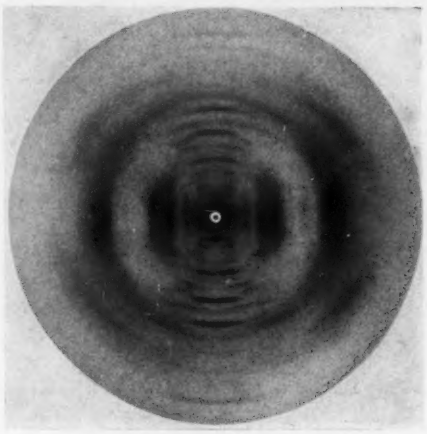
Figure 10 – The quill of a seagull's feather. A pattern of this kind is common to all feathers and beaks of birds, tortoiseshell, reptiles' scales, etc. It lays bare in a strikingly fundamental fashion the phylogenetic affinity of birds and reptiles as distinct from the mammals.

Figure 12 – An artificial silk, or *rayon*, made from regenerated cellulose. The chain-molecules lie side by side just a little differently from what they do in native cellulose fibres. The occurrence in the photograph of so many and such sharp spots instead of arcs is a measure of the perfection and strength of this particular rayon.

Figure 13 – *Nylon*, the new and truly synthetic fibre (a polyamide) that is now rivalling natural silk. It is a plastic, the fibres or bristles being spun from the molten state.

Figure 14 – The mineral fibre, *chrysotile asbestos*. It is a silicate (a fibrous serpentine) constructed from long chains of linked (SiO_4) groups. The fact that certain spots are drawn out into streaks along the hyperbolae shows that the sideways distance between the chains is variable, on account of variations among the metallic ions that lie between them and hold them together.

FIGURE 9



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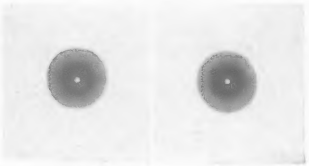


(*a*) (*b*)

FIGURE 11

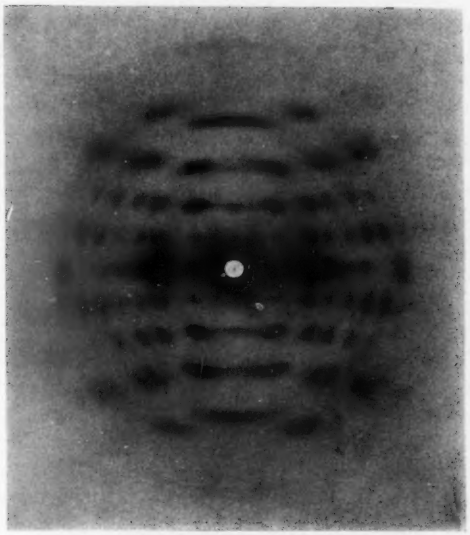


FIGURE 12

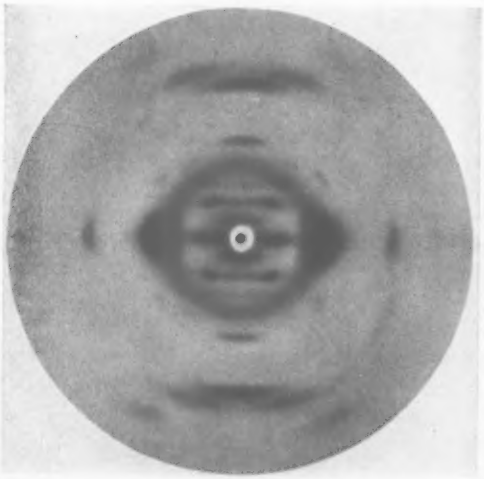


FIGURE 13

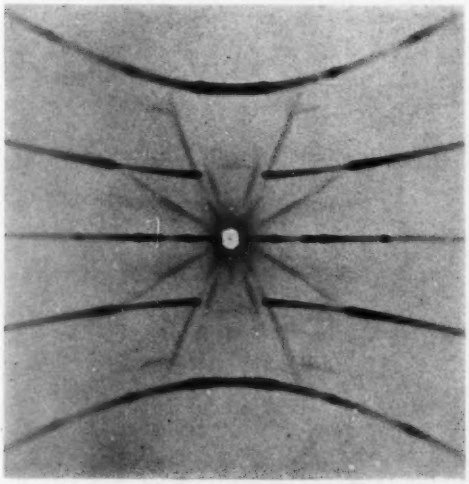


FIGURE 14

The action of X-rays on living cells

J. A. CROWTHER

Modern science attacks specific problems from many different directions simultaneously. It is probably this method of concerted assault which, more than any other factor, has led to the remarkably swift progress of the last half-century. In the following pages, Professor Crowther shows how investigation of the action of X-rays on living cells may throw considerable light on the mechanism of heredity.

The discovery of X-rays was one of those germinal events in the history of science which, important in themselves, gain an increased significance as opening up fresh opportunities for further advances in many directions. The new tool was immediately recognized by the medical profession as a powerful aid to diagnosis, and the science of medical radiology has now reached a high pitch of perfection. Somewhat more tardily the engineer has come to adopt similar methods for testing the homogeneity and freedom from defect of the materials with which he works. In aeronautical engineering in particular, where considerations of weight make it necessary to trim down the margin of safety to its narrowest permissible limit, a radiographic test of important welds and castings is becoming a matter of routine. In an entirely different field the application of X-ray methods to problems of crystalline and molecular structure has led to notable advances on both the theoretical and the practical sides.

It is now becoming probable that X-ray investigations may prove at least as fertile when applied to some of the more obscure problems of biology. The territory lying between physics and physiology is certainly one of the richest of the as yet unexplored fields of science. Its successful exploitation requires close co-operation between the biologist and the physicist—a combination not easily secured. The progress that has been made so far, in this country, owes much to the far-sighted policy and generous assistance of the British Empire Cancer Campaign.

The action of X-radiation on living cells is invariably destructive. The therapeutic value of the radiation, which, in spite of some disappointments, is by no means small, is due to the fact that diseased and abnormal cells are more sensitive to radiation than the healthy tissues by which they are surrounded, and may be destroyed by doses of radiation which leave the healthy tissues unharmed, or at least not damaged beyond recovery. The extraordinary differences in radio-sensitivity of tissues of different kinds, and of cells of the same kind at different stages in their history, is a striking phenomenon, for which any theory must be prepared to account. Still more striking is the minuteness of the dose which, in certain cases, is sufficient to produce a marked biological effect. The mean lethal dose for the eggs of the blow-fly is only some 40 units, corresponding to a liberation of energy in the egg of rather less than one ten-thousandth of a calorie per gram.

A detailed study of the lethal action of continued irradiation on large populations of similar cells yielded even more surprising results. It is a matter of common observation that individuals in a population exhibit very different powers of resistance to adverse circumstances, such as exposure to extreme cold, to starvation, or to infection. A few weaklings may expire in the early stages of exposure, but the death-rate does not become serious until the onset has been so prolonged that it exceeds the average power of resistance of the individuals. The percentage of survivors then begins to fall rapidly. Even so, however, there will be certain particularly hardy individuals with powers of resistance well above the average, so that exposures much greater than the mean will be

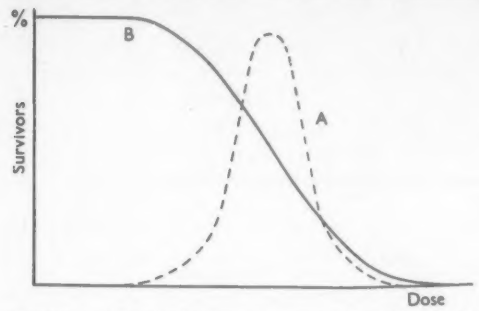


FIGURE 1 – *Survival curves for symmetrical distributions of vulnerability.*

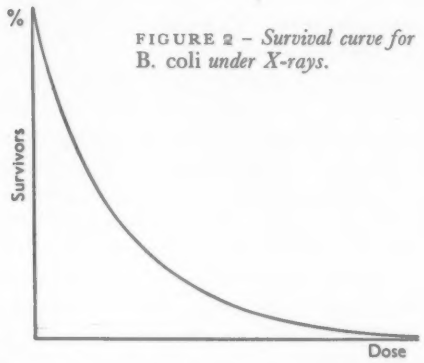


FIGURE 2 – *Survival curve for* B. coli *under X-rays.*

required to complete the process of extermination. Thus if we plot a graph showing the percentage of the original population surviving a given length of exposure, the resulting curve, known as a 'survival' curve, will be sigmoidal, as indicated by the full line in figure 1. The broken line indicates the distribution of susceptibility among the individuals in the population. The sigmoidal nature of the curve is simply an expression of the fact that some members of the population are more, some less, susceptible than the average.

Survival curves approximately of this type are not uncommon when the lethal agent is X-radiation. With X-rays, however, there are also many now well-authenticated cases where the survival curve takes quite a different course (figure 2). The number of deaths is actually at its highest at the first onset of the radiation, becoming progressively less as the exposure is increased. To explain this as a result of biological variations among the individuals we should have to assume not only that the sensitivity of the cells ranged from infinity to zero, but also that the largest group of the population is the one with the minimum of resistance. Such a distribution of sensitivity has never been found towards any other lethal agent. Moreover, close investigation reveals that this new type of survival curve is actually of the type known as an exponential. It indicates that whether exposure to the radiation has been slight or whether it has been prolonged, the death-rate remains absolutely constant. An individual bacillus, say, which has already been exposed to a considerable dose of radiation is, if it is still alive, no more likely to succumb to a further dose of radiation than one which has suffered no radiation at all. It is just as if a person, if deprived of further food for another day, were as likely to succumb to starvation immediately after a good dinner as after a three weeks' fast. For the resolution of this paradox we must look, not to the nature of the biological material, but to the nature of the lethal agent.

It is natural to imagine that in an object which is exposed to a beam of X-rays every individual molecule is subjected to the action of the radiation. It is, in fact, rather difficult to disabuse one's mind of this idea of uniform illumination. The idea is, however, very far from the truth. A beam of X-rays consists in fact of a swarm of *photons*, particles of energy of very minute size, and the absorption of X-radiation in matter consists in the stoppage of one or more of these particles by a single atom of the substance. The atom disposes of the energy so absorbed by ejecting a high-speed electron, which, in turn, ionizes a fraction of the molecules through which it passes, leaving a trail of charged molecules or ions in its wake, until its original energy is all dispersed. This process, which has been extensively studied, is very well illustrated by the photograph, due to C. T. R. WILSON, reproduced in figure 3. The photograph represents the passage of a beam of X-rays through air, from right to left of the

FIGURE 3 – *The passage of a beam of X-rays through air.*

picture, and the individual ions have been rendered visible by depositing a drop of water on each—an invaluable technique originated by Wilson. It is obvious, from the picture, that the energy of the radiation has been concentrated on an extremely small number of molecules, the majority being completely unaffected by the rays. It is important to realize how highly discontinuous the action of the radiation can be. A particle of diameter as big as 0·2 μ, which would be distinctly visible in a good microscope, would stand a fifty-fifty chance of being untouched by a dose of 100 units; the passage of a beam of X-rays at an intensity of 1 unit per second for 500 years would still leave one-third of the individual molecules unscathed.

Now the chance that someone will be hit by a bullet fired at random is obviously proportional to the number of persons within range; and the number killed in a given time during a random attack will (assuming that every hit is fatal) be directly proportional to the number exposed to the attack. If the attack is continued the death-rate per thousand among the survivors remains constant, and the survival curve is exponential. Thus the exponential survival curves obtained when certain bacilli are attacked by X-rays, so mysterious when viewed from the biological point of view, are exactly what we should expect from the physical nature of the radiation, on the not unnatural hypothesis that the liberation of energy in some vital part of the structure of the organism is sufficient to cause death. The same hypothesis also affords a natural explanation of the observed independence of the lethal dose on the rate of irradiation, since the number of victims obviously depends only on the number of bullets fired, and not on the rate of firing.

The 'target' theory of the action of X-rays, as it is called, has not lacked influential critics. While the theoretical argument can hardly be controverted, it was suggested as recently as 1937 by SCOTT that the experimental evidence was insufficient to establish the exponential nature of the curves, and that they might be merely sigmoidal curves, with a very short initial flat portion. Brilliant experimental work by TIMOFEEF-RESOVSKY on the effect of X-radiation on gene-mutation, and of WYCKOFF on the lethal effect of the radiation on bacteria, has established the shape of the curves, for their particular material, beyond reasonable doubt. The 'target' theory is now accepted by most workers in the field. It is the only one which offers a rational explanation of the facts, and has the additional merit that it opens up a new avenue of approach to some important biological problems.

Let us assume that, in colonies for which the survival curve under radiation is of the exponential type, an individual will be killed if an ion is formed within some vital structure, a suggestion which I put forward in 1924 in connection with some early experiments of STRANGEWAYS in Cambridge on tissue cultures. Actually X-rays have a tendency to produce ions, not singly, but in very minute clusters, the average number of ions in a cluster being three. The volume of the sensitive region is obviously the volume in which the mean lethal dose has a fifty-fifty chance of producing one of these clusters of ions. Since the total number of clusters formed in unit volume by a given dose of radiation is known from physical measurements, the average volume which will contain one cluster can be calculated very easily. It is always much smaller than the volume of the whole cell. Thus X-rays act, not on the cell as a whole, but on certain specific structures within it. In many cases these structures turn out to be well below the resolving power of even the best microscope. It is obvious that X-rays open up an interesting line of investigation into the sub-microscopic structures of the living cell. Where the effect of the radiation is sufficiently drastic to produce the death of the organism, it may be difficult to identify the target with a known structure in the cell. Where, however, the immediate effect is the production of a 'mutation,' the structure can with some confidence be identified with a 'gene.' This line of investigation has been followed up energetically by Timofeef-Resovsky and others, with illuminating results. The same method has also been applied with some success to phages and viruses.

An interesting extension of the principle has recently been described in papers by LEA (of the Strangeways Laboratory) and HAINES, who have made an extensive study of the effect of radiation on the colon bacillus. It is well known that the density of ionization along the

FIGURE 4 – *The track of an α-particle in air.*

Photograph by Professor C. T. R. Wilson.

Reproduced by courtesy of Edward Arnold & Company.

track of the photo-electrons ejected by X-radiation depends on the wave-length of the radiation. For short waves the density is small, for long waves the ion clusters are much closer together. The density becomes even greater if material particles such as the α-particles from radium are used as the ionizing agent. It can be seen from figure 4, which represents the track of an α-particle in air, that the ion clusters are so closely packed as to be indistinguishable in the photograph, and show as a continuous streak. The distance between the clusters formed by α-rays is in fact much smaller than the linear dimensions of the target volume of *B. coli*, as deduced from X-ray measurement, so that, whereas the X-ray leaves only one ion cluster in the structure, the α-particle might be expected to leave, perhaps, eighty. But since one cluster is sufficient to immobilize the cell, the remaining seventy-nine are completely wasted. Thus if both radiations are measured (as all ionizing radiations are now measured) by the total number of ions produced, α-rays should appear to be only one-eightieth as effective as short-wave X-rays. Direct comparison of the effects of different types of radiation on the colon bacillus demonstrated quite clearly that the lethal efficiency did in fact decrease as the ion density along the particle track increased, but by no means on the scale expected. The efficiency of the α-particles was in fact about one-eighth, not one-eightieth, of that of the short-wave X-ray.

To explain the discrepancy Lea has made the suggestion that the sensitive volume of the colon bacillus is not a single particle but is divided into a number of smaller particles, a hit on *any one of which* is sufficient to produce the ultimate death of the cell. Suppose, for example, that the sensitive volume is distributed among a thousand individual particles, the total volume remains the same, but the linear dimensions of each particle will be reduced to one-tenth. The chance of a hit remains the same as before, since it depends only on the total sensitive volume, but the number of ions left behind by an α-particle as a consequence of a hit is now only eight and not eighty times the number due to the X-ray, as required by the observations. The diameter of the particles was calculated to be 8·6 mμ.

Admirably as the suggestion meets the physical requirements of the problem, it is necessary to scrutinize it from the biological standpoint. Our target particle, of which there are about a thousand in a single bacillus, has a volume which would not be impossibly large for a single molecule, of molecular weight about 200,000. The particles are, presumably, of many different kinds, since it seems improbable that the destruction of one out of a thousand identical molecules could seriously inconvenience the cell. Moreover, since a single hit will ultimately cause the death of the cell, each particle is one which is vital for its proper functioning and one it is unable to synthesize again if it has once been destroyed. Since the organism cannot manufacture the particles it must presumably have inherited them, and must therefore be capable of duplicating them during the process of cell-division, so that each daughter-cell may have its full complement. This set of requirements appears to be quite fantastic until we realize, as Lea points out, that 'these are just the properties which current theory attributes to the *gene*.' If we regard a gene as a definite particle rather than a mere position in the chromosome, it would exhibit precisely that curious combination of properties which theory bids us postulate in our X-ray targets. The two best studied effects of X-rays are their ability to produce mutations and their power to kill. The experiments described give strong evidence that in each case the 'target' for the radiation is the same.

These experiments provide a straightforward and interesting illustration of the potency of X-radiation in the investigation of what may be called the fine-grained structure of matter: the region which lies between the atomic (about which much is known) and the actual

microscopic. Thus though it is doubtful whether the existence even of chromosomes in *B. coli* has yet been demonstrated by the microscope, X-ray methods have enabled Lea and Haines to state with much probability that the bacillus contains some thousand genes of average diameter 8·6 mμ. It is beginning to be realized that in the inorganic world, as well as in the organic, 'fine-grain' particles may play a more important part than we had hitherto suspected. The colloidal state of matter owes its peculiar properties to particles within this range, and it is suspected that they play a part in many metallurgical phenomena.

Nothing has yet been said about the mechanism by which the production of an ion-cluster in the target particle brings about its lethal effect. DESSAUER many years ago suggested that this concentration of energy resulted in the formation of 'hot-spots' within the organism. Lea and his collaborators at one time suggested that the ions initiated a chemical reaction producing some poisonous substance in the neighbourhood of the target. They would possibly now regard it as being due to a rearrangement of the gene structure. It has always seemed to me to be possible that the action may be even simpler and more direct. Most of the structures in a cell-nucleus are known to be electrically charged, and, in particular, the phenomena of localized points of attachment in the chromosomes is strongly suggestive of the existence of localized electrical charges at various points of the structure. The production of a cluster of ions in the vicinity of these charges would lead to their neutralization, which, if we are right in suggesting that the attachments are electrical in origin, must cause a break-up of the system. Fragmentation of the chromosomes is, in fact, a common effect of X-radiation. Experiments which have been in progress in Reading for some time on inorganic colloidal solutions have demonstrated quite conclusively that both the mobility and the charge on the minute particles which make up these solutions can be profoundly and permanently altered by doses of X-radiation which are quite small, even when compared with those required to produce biological effects. Making full allowance for the natural tendency of a physicist to seek for explanations within the terms of his own science, the possibility that the biological effects of X-radiation result from a direct electrical action of the ion cluster which, as we have seen, constitutes a 'hit,' seems worth further investigation.

Recent work

THE PHYSICIST AIDS THE PHYSICIAN

Contributed by D. S. Evans *and* K. Mendelssohn, *Oxford*

It has been found possible to make considerable improvements in long-established medical practice when once the underlying physical principles were understood. One example of this work is the development of a vaporizer for the quantitative administration of ether vapour as an anaesthetic. By use of the familiar principle of the chemical thermostat the evaporation is carried out at a constant temperature. The vapour is thus kept at a constant pressure and can be mixed with controlled volumes of air or oxygen by the use of a simple mixer tap. The heat source for the vaporization is boiling water, which means that the operation of the machine is so simple that it is well adapted for use even under the difficult conditions of the field hospital or the casualty clearing station. The anaesthesia produced is always under perfect control, and may be conducted under medical supervision by a comparatively unskilled person. This type of machine is to be distributed to the British Army, and has in addition a very obvious peace-time value. An extension which is envisaged is the development of a simple apparatus for producing analgesia for use in the dressing of severe wounds and in childbirth.

Another example is the development of a simple semi-automatic device for the registration of blood-pressure and blood-flow in a limb during long operations. By means of a direct visual recording, a continuous indication is given to the anaesthetist and surgeon of the amount of blood flowing through the limb with each heart-beat, thus enabling an early diagnosis of shock to be made, and allowing remedial measures to be instituted before this dangerous condition is so far advanced as to be intractable.

Studies have also been made of the efficacy of radiant heat cradles for the treatment of shock, and it has been found possible so to modify their design that the rate of warming of the patient is greatly increased, while at the same time the risk of burning is decreased. The many possible applications of thermo-electric thermometers in medical practice are being explored, and a simple instrument has been devised for the detection of dangerous swelling of injured limbs encased in closed plasters.

The considerable progress which has been made in a short time is due to the application of physical methods by physicists to selected fields of medical practice, with the constant advice and assistance of medical specialists of great clinical experience.

Phthalocyanines

C. J. T. CRONSHAW

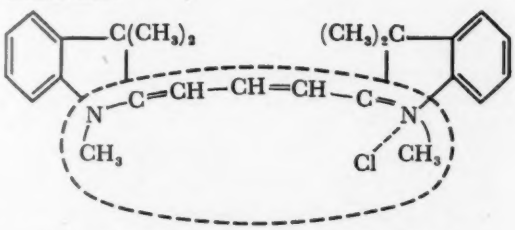
The search for new and better colouring matters is as old as history. Dr Cronshaw relates how examination of an accidentally discoloured batch of phthalimide led to the discovery of one of the most valuable of all known classes of pigment.

Fundamentally the organic chemist in his study of the relation of chemical constitution to physical properties is a designer and erector of buildings appropriate for particular purposes. He is therefore just as much concerned with problems of shape and size, of stresses and strains, of angles and dimensions, as is the architect proper; there are, too, both beauty and inspiration in such structures for those capable of appreciating the science. Moreover, the 'buildings' of organic chemistry are in principle, though more rarely in practice, erected from elementary units just as the builder lays brick by brick. This planned and deliberate synthesis of molecules is characteristic of organic chemistry, in distinction from inorganic chemistry.

The organic chemist in the practice of molecular architecture has given a particular name to those parts of the edifice which introduce the characteristic of colour; he calls them *chromophores*. A coloured substance is not necessarily a dyestuff—the latter results from the addition to the structure of certain other groupings in addition to the chromogenic part of the molecule—but alike for colour and for dyeing power the prime necessity is the chromophore.

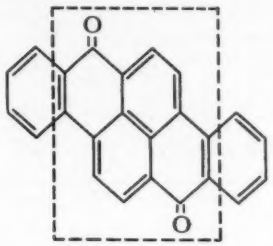
The suggestion that the presence of chromophores within the molecule is responsible for colour in an organic chemical substance is an old one and was made by WITT in 1876. Whilst its validity remains true, the passage of years showed that it was perhaps only part of the story and required an additional conception. This we owe to that revered and dynamic personality, the late H. E. ARMSTRONG, who showed that many of the coloured substances then known had a structure similar to quinone, which itself is a pale yellow-coloured substance. That was in 1888. Quinone is a simple derivative of benzene and has two atoms of oxygen which create in the chemical structure a series of alternative stresses, and it is the existence of this conjugation of unsaturation within the molecule which gives rise to coloured substances. Recent work has added a further refinement in that whilst this chain of stresses preserves a sequence of 'stress' and 'no stress,' the particular incidence of any stress is not constant but oscillates and gives rise to what is called *resonance*. Actually the theory assumes an intermediate stage between the two changes as being the really effective state, much as a journey may start at A and finish at B and the rate of the journey be so incalculably swift that no traveller has ever seen the intermediate places which lie between the terminal points.

There are, of course, many rules in the technology of dyestuffs governing the relationship between constitution and physical properties, since so much else is required as well as the property of colour—for example fastness to light, an appetite for certain fibres, etc.; but whilst the requirements are infinitely varied this matter of the chromophores remains the heart and kernel of the whole affair. The nitro group NO_2 is a chromophore, and so is the well-known azo grouping —N=N—, which is, so to say, the single theme upon which the largest single family of dyestuffs is based. These examples are within the simplicity of the chromophores as originally conceived by Witt; other examples are more complicated arrangements like:



The portion within the area enclosed by dotted lines is the chromophore.

which is the beautiful pink dyestuff called Astrophloxine, and



The portion within the area enclosed by dotted lines is the chromophore.

which is the yellow compound called dibenzpyrenequinone.

Both these examples show a complexity beyond the original view of Witt, and fall within the modern conception of colour.

It is worth remarking in passing that just as the professional architect has his long row of cottages and his storied and more complicated edifices, so the chemist has simple compounds built up from long chains of single carbon atoms ('ribbon development'!), and also intricate compounds where carbon atoms are joined into a six-membered ring or even several such rings coalesced together.

The number of coloured organic chemical products is extremely great; the number of possible dyestuffs must run into a million or so; and the number of dyestuffs which by reason of special properties are commercially available is probably between two and three thousand. On the other hand, the number of chromophores is relatively much smaller. And whilst individual dyestuffs may lose their utility and become obsolete, invention and research more than balance the wastage, so that the total number available still grows. Indeed, an increasing demand for diversity and range of dyestuffs seems to go hand in hand with the progress of civilization. Even so, the discovery of a single new dyestuff usually ranks as an important event in both the science of organic chemistry and the technology of dyeing and printing. The discovery of a new chromophore is of course a much rarer occurrence, and always an important and exciting one, since it should open up a vast new continent for scientific exploration and technical endeavour. When, therefore, the constitution of the recently discovered phthalocyanines, published in the *Journal of the Chemical Society* in 1934, revealed the existence of a new chromophore the interest aroused was both natural and widespread.

This new chromophore fits with precision into the modern conception of colour and affords a splendid example of resonance. Although the existence of the phthalocyanines was not predicted, and was perhaps not even predictable, yet now the discovery has been made and the structure of the molecule ascertained, no one can fail to remark the inevitability of the compound. Its right to existence is almost declamatory! It is remarkable how readily at the appropriate temperature of reaction, and in the presence of a metal such as copper, the four integral components almost snap into position.

Phthalocyanines, themselves entirely products of the chemist's skill, have resemblances to two important substances which occur abundantly in nature. The first is chlorophyll, which is the green colouring matter in plants; and the second is haemin, which is the red colouring matter in blood. Both of these play an important part in the cycle of life in the two kingdoms, vegetable and animal, in which they are respectively so widespread. Whether this relationship implies any profound physiological properties for future members of the phthalocyanine series cannot be foreseen, and must await further scientific exploration. Both chlorophyll and haemin, however, are highly sensitive substances easily destroyed by light, heat, and even mild chemical reagents. Phthalocyanines, on the other hand, withstand the action of light and are amongst the 'fastest to light' substances known. They resist heat so well that the copper compound will sublime at about 600° C. without decomposition, a property which in itself is sufficiently remarkable to render the phthalocyanines unique in this respect among organic compounds. Phthalocyanines can be dissolved in concentrated sulphuric acid and reprecipitated without injury; indeed, this property is the basis of the method used on the commercial scale for their purification. They are insoluble in water and in nearly all organic solvents; and their resistance to the action of light, to organic chemical solvents, to heat, and to most acids, coupled

with their great beauty and intensity of shade, put them into the front rank as pigments.

The phthalocyanines possess a further property which admirably adapts them for the special printing ink required for the so-called three-colour process used in colour printing. This in essence consists in making three printing plates, prepared by photographing the picture to be reproduced through selective colour filters in turn. Such plates are capable of printing as single images all the yellow, all the red, and all the blue portions of the picture. If prints are made from each of these plates (due care being paid to proper coincidence of the three images), so that each printing is superimposed—first the yellow, next the red, and finally the blue—a reproduction in full colour of the original picture results. All these processes, of course, call for great skill and appropriate materials, but it is essential that the yellow, the red, and the blue inks used shall be as nearly pure yellow, pure red, and pure blue as possible. If, for example, the yellow is an orange, that is yellow + red, or a greenish yellow, that is yellow + blue, or if the blue is a purple-blue, that is blue + red, then these secondary and adventitious shades are misplaced on the plates and when superimposed lead to dull reproduction.

Excellent trichromatic yellow and red pigments have existed for some time, but the full beauty of reproduction theoretically possible by use of the three-colour process could not be realized in practice by the use of ink based on existing blue pigments. The discovery of the phthalocyanines removed the remaining flaw in this method of colour reproduction. Monastral Blue (the trade name for the copper phthalocyanine compound), because of its purity of shade, is a perfect blue pigment for this purpose, and the comparative plates illustrated on page 83 show the extra range and clarity obtainable by the use of printing ink containing this pigment.

The phthalocyanines have, of course, all the range of utility for which pigments as a class can be employed. The only two blue pigments previously available with the characteristic of exceptional fastness to the action of light were Ultramarine and Prussian Blue, both well remembered by everybody as inhabitants of the box of paints in childhood days. Both have, however, serious shortcomings from which the copper phthalocyanine does not suffer: Prussian Blue is unfortunately damaged by alkalis and Ultramarine by acids.

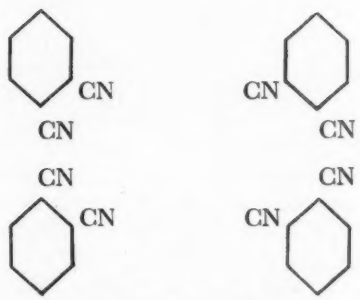
The story of the discovery and the patient and skilful unravelling of the structure of the phthalocyanines is a splendid emblem of the happy marriage which has existed between the pure science of organic chemistry and the technology of the British dyestuffs industry, a union whereby a full and abiding partnership in observation and research can make invention a continuing reward.

The first member of the series was discovered by chance in 1928. It was observed that the manufacture of phthalimide in the Grangemouth works of Scottish Dyes resulted on occasion in a number of batches being slightly discoloured, and the phthalimide, therefore, was unsuitable for its proper purpose. Investigations on the spot by DUNWORTH and DRESCHER showed that the discoloration was caused by the presence of traces of a dark blue substance which was apparently formed during the reaction of molten phthalic anhydride with ammonia. The cause of this trouble having been ascertained, it was only necessary to adjust the conditions of manufacture so as to eliminate the undesirable impurity. There the matter might well have ended, but a detailed examination of the isolated impurity revealed too many marks of potential interest for the substance to be dismissed lightly from consideration. It was very stable and crystalline, and contained iron, which was apparently firmly entrenched as part of the molecule since it was not readily eliminated by the customary means. Moreover, and more important, it was a dark blue substance, and perhaps this was safeguard enough and to spare, in a dyestuffs works at any rate, against any tendency to allow such a substance to escape further and closer examination. The fact that it was coloured and insoluble in water instantly suggested its use as a pigment, but despite much experimentation and consideration nothing of utility resulted. A tribute is therefore merited to the people concerned, who despite the lack of promise had sufficient faith and interest in this novel substance to pursue the matter further. At the suggestion of the Research Committee of I.C.I. (Dyestuffs) Limited, DR LINSTEAD, of the

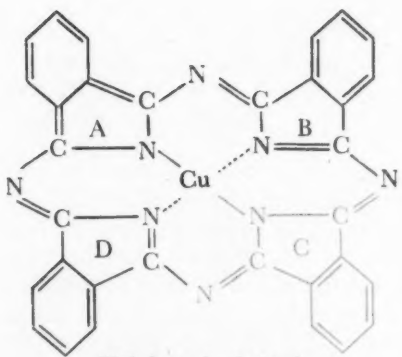
Imperial College of Science and Technology, London, took up the scientific study of the compound. To him also we owe the name which signifies both the origin and the deep blue colour.

The problem of the chemical constitution of the phthalocyanines was solved by Linstead and his co-workers, and was reported in the *Journal of the Chemical Society* in May 1934. From this date to 1939 the same journal contains a sequence of papers, including an X-ray confirmation of the structure by ROBERTSON, on these interesting compounds. The first member of the series, Monastral Blue B, was placed on the market in 1935. This peep behind the scenes illustrates the long, certainly active, and perhaps inevitable period of gestation required from conception to birth, even of a scientific discovery.

The chemical constitution can be considered as being built up out of four molecules, without loss, of phthalonitrile round a central nucleus which may either be two atoms of hydrogen or an atom of almost any metal. Thus:



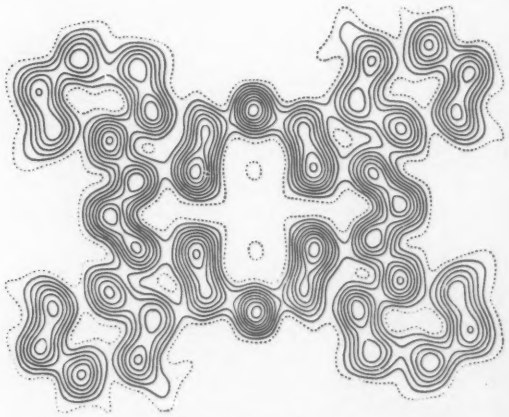
Four molecules phthalonitrile.



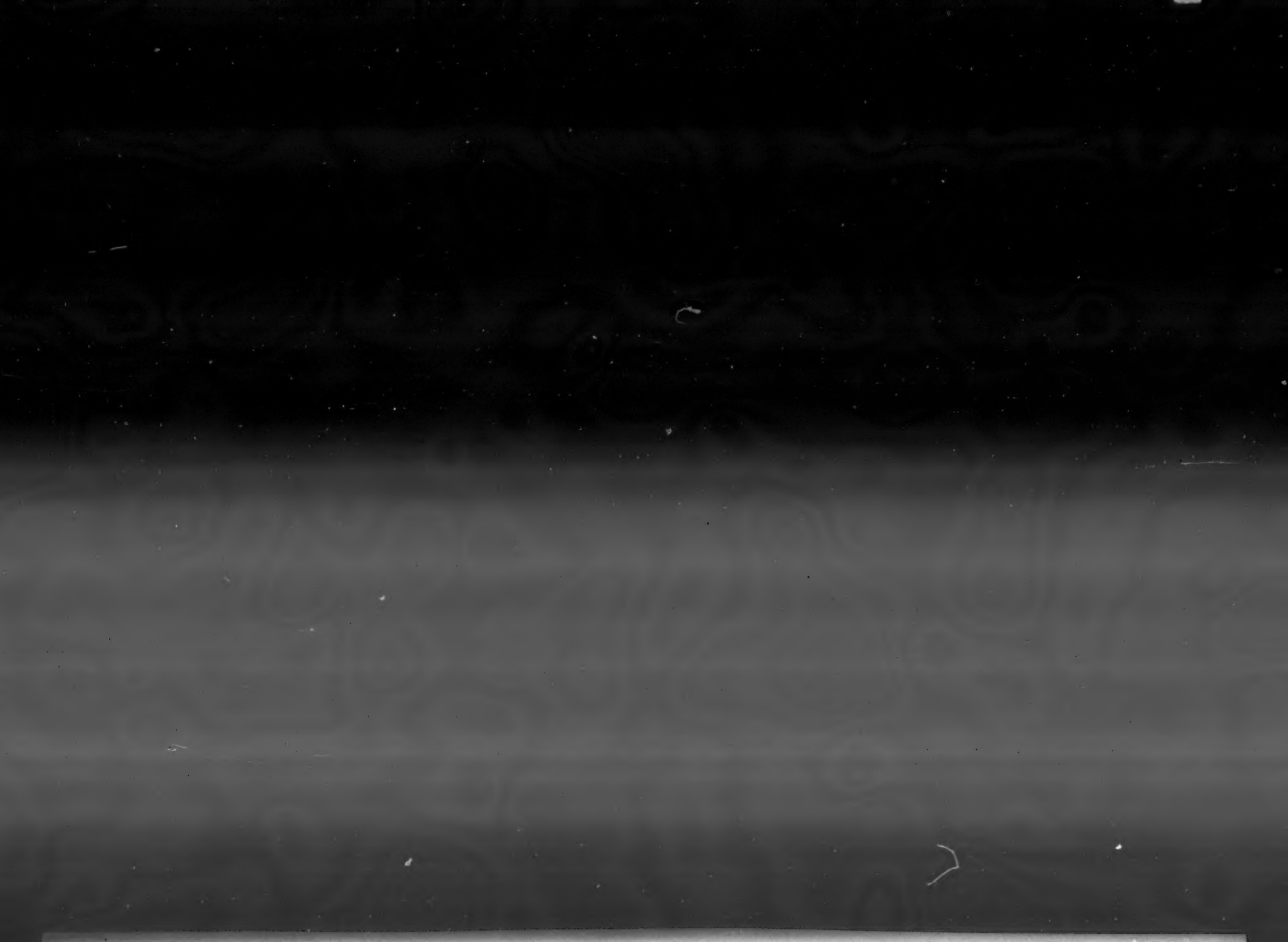
Phthalocyanine (copper).

In this representation the rings A, B, C, D differ in arrangement of 'stress' (indicated by the double bond) and 'no stress,' but the chemist believes that this represents, so to speak, only one snapshot in a cinematographic sequence, and that the difference is not permanent since all four rings successively take on all the structures in rapid alternation. This resonance in the structure accounts, according to modern conceptions, for the outstanding stability of these substances and possibly also for the colour. All the spokes uniting the copper hub to the external structure must be regarded as identical and equivalent, and it is a remarkable fact that two hydrogen atoms can replace the copper atom without loss of stability. This observation may stimulate modern work on co-ordinated hydrogen, an idea which was inconceivable a few years ago.

The scientific interest which the discovery of the compounds has aroused is by no means exhausted. For example, the metallic derivatives, particularly the one containing iron, have shown remarkable catalytic properties. Then, too, all members of the known series are either blue or green; but the dyestuffs chemist cannot readily accept such a restricted palette of colour as being the complete heritage resulting from the discovery of a new chromophore. Nor can the phthalocyanines be used, as yet, as dyestuffs for textile fibres. And so the work goes on, in the hope that perhaps only the first chapter of the full story is at present available.



An X-ray shadow picture of a phthalocyanine molecule.



Comparative plates showing the extra range and clarity obtainable by the use of printing ink made with a phthalocyanine compound. The same set of blocks is used for both pictures: although in practice the photo-engraver would make some corrections in etching to compensate as far as possible for inaccuracies in the standard pigment.

(*a*) *Printed with a trichromatic ink made with Monastral Blue pigment.*

(*b*) *Printed with the British Standard shade of trichromatic blue.*

Vegetable drugs of Britain

and their exploitation in the war

R. MELVILLE

Great Britain has in the past imported from foreign countries materials that could have been grown at home. War has altered the picture, and, though deprivation may be irksome while it lasts, necessity has at least the merit of fostering our native resources. Dr Melville describes a case in point, and makes it clear that we can go far to grow our own needs of vegetable drugs.

The war has affected imports of all classes of merchandise. Imports of drugs, although considerable, form a relatively small part of our annual trade, but owing to their necessity for the national health bear an importance out of proportion to their size. Many drugs of tropical and subtropical origin must necessarily be imported, quite a number from temperate climates can be cultivated in these islands, and some are native. Although very few British plants are included with the more highly potent and well-established drugs of the *British Pharmacopoeia*, among them belladonna, henbane, digitalis, colchicum, and valerian, our ancestors made use of nearly every plant in the flora, often ascribing to them virtues that were non-existent and based upon some fancied resemblance to the organ they were supposed to remedy. Many of these have been discarded on account of the discovery of more potent remedies. Thus, comfrey root, long used in the treatment of wounds and ulcers, contains allantoin which stimulates cell-growth, but which is readily synthesized and, with other cell-proliferants, replaces the crude drug. Nevertheless a trade still exists in upwards of eighty native plants in amounts varying from a few hundredweights to many tons per annum.

Far the greater part of our supplies of these native drug-plants were imported from the Continent, often when abundant supplies could be grown at home for the trouble of harvesting. An outstanding example is dandelion root, of which about 250 tons per annum are used. As the reasons for this state of affairs were economic, not botanical, it should be possible as a war-time measure to organize the collection of some of the more important and extend the cultivation of others. Much has already been done on these lines. The area now under belladonna should provide for all, or the greater part, of domestic needs of leaf in a normal year, though root is likely to be scarce for some time. The cultivation of henbane, digitalis, and several other drugs has been extended, but more could be done with drugs requiring short-period cultivation such as caraway and valerian, as well as the aliens dill, lobelia, and psyllium. Uncertainty as to post-war conditions and difficulty in obtaining planting stock militate against much extension of cultivation of drugs requiring a long maturation period. Colchicum and male-fern do not lend themselves readily to cultivation on account of the small return obtainable for the long periods they take to mature, and both are normally collected in the wild. It does not seem likely that such plants can ever be grown economically except perhaps as a catch-crop among other long-term crops. Male-fern can be propagated by cutting off the crowns of large rhizomes for replanting when the rhizomes are dug. About ten years are then required to produce another rhizome 6 to 8 in. long. An opportunity exists for an enterprising landowner in the West of England to experiment with planting male-fern in mixed woodlands bearing not too dense a canopy, as the demand for this specific against tapeworms is likely to continue unabated.

The garnering of native British drug-plants may seem a simple business, but war-time collection is beset with difficulties at every stage.

Transport for the collectors and the herbs gathered is hard to obtain; drying sheds requiring timber and other materials are a prime necessity and must be fitted up, while fuel is needed to provide heat to complete the drying in dull weather. Restrictions on all these commodities have hampered the work of the voluntary organizations assisting in the work of collection. An account of the steps taken to instruct collectors and other details of organization has been published elsewhere (*Pharm. J.*, 1941, **146**, 213).

Left: Meadow saffron in an open wood on the Cotswolds (Colchicum autumnale).
Right: Foxgloves on clay with flints, Surrey (Digitalis purpurea).

The Therapeutic Requirements Committee of the Medical Research Council has issued a list of twenty-six drugs recommended for increased production in the United Kingdom, of which eleven vascular and four non-vascular plants are native. Not all of the natives are abundant enough for collection. Belladonna was almost exterminated during the last war and its re-establishment was not encouraged on account of the danger of poisoning to domestic animals, so that few places remain where it can be collected. Henbane is rarely seen except as a casual; the wild forms of *Anthemis nobilis* and fennel are not those used in medicine. Valerian is scattered and difficult to collect in quantity; it is cultivated on a small scale and cultivation should be extended. On the other hand, colchicum is locally abundant on calcareous soils in the West of England, and considerable quantities of corms and seeds have been collected. Colchicum was formerly much more widespread and abundant, but the Ministry of Agriculture's campaign against it as a noxious weed has resulted in its extermination in many localities. The foxglove, *Digitalis purpurea*, is abundant and widespread on siliceous soils, including the clay with flints overlying chalk. The principal difficulty is to dry the leaves rapidly enough with the makeshift appliances voluntary workers have at hand, to preserve the bulk of the very unstable glycosides on which the medicinal activity depends. It is not possible to send fresh leaves by public carriers to distant drying centres, as drying must be started with as little delay as possible. Not nearly enough of this valuable drug was collected in 1941 to satisfy the demand. Digitalis seeds are used for the preparation of the glycoside digitalin.

There is much to be said in favour of a simple bitter for stimulating the appetite in dyspepsia and convalescence, and for this purpose dandelion (*Taraxacum*) might to a large extent replace gentian, supplies of which are difficult to obtain. Another common weed in great demand is the stinging-nettle, required for the preparation of chlorophyll. The pigment is extracted more readily from stinging-nettles than from other plants and is of a finer colour.

A number of other British plants are used medicinally, mainly by herbalists, in quantities of 1 to 10 tons per annum; but of more immediate interest is the prospect of preparing a satisfactory substitute for agar-agar from native seaweeds. Carrageen, *Chondrus crispus*, yields a mucilage widely used as an emulsifying agent. An agar-like substance can be prepared from it suitable for many industrial purposes, but not for bacteriological media. A wide range of algae has been tested but only the red seaweeds *Gelidium* and *Ahnfeldtia* have yielded agars of bacteriological quality. An ecological survey is now being made to determine whether adequate supplies for manufacturing are available round our coasts.

Book reviews

COSMOLOGY FOR EVERYMAN

The Birth and Death of the Sun, *by George Gamow. Pp. xiv + 238. Macmillan & Company Limited, London.* 1941. 12*s.* 6*d. net.*

It is not difficult to understand why the majority of laymen, when they are interested in science at all, find a peculiar attraction in cosmological problems. Such problems impinge at many points upon the general philosophy of life, and although the average man's urge to inquire into the ultimate nature of things may be only desultory, yet at one time or another 'everyone has feebly shaken with his hand the bough of truth.' This impulse, no less than the skill of the author, accounts for the popularity of *The Universe around Us* (SIR JAMES JEANS) and similar books. PROFESSOR GAMOW'S account of modern scientific views on the birth and death of the sun, on stellar evolution, and on subatomic energy would therefore be certain to find readers even if it were only mediocre. Far from being mediocre, however, it is among the best of its class, and if Jeans and Gamow rub shoulders on the bookshelf they will be good companions. But we are not convinced that the object illustrated in figure 7 of the book, and described by Professor Gamow as 'an ancient Persian electric battery' probably of the first century B.C., has any authentic claim to that identification.

THE BASES OF THERMODYNAMIC THEORY

The Nature of Thermodynamics, *by Professor P. W. Bridgman. Pp. xii + 229. Oxford University Press, London (Harvard University Press).* 1941. 20*s. net.*

PROFESSOR BRIDGMAN has already written on the logic of physics and on a particular application of thermodynamics, so that he is particularly well qualified to undertake an examination of the underlying bases of thermodynamic arguments. His outlook has evidently been influenced, consciously or unconsciously, by the deep analysis of physics which has been made in the past decade in connection with the newer mechanics. Nearly all discussions of this nature, whatever their subjects, end up by confirming the correctness of the procedure commonly used, and thus do little more than provide a secure foundation for a structure which they do not disturb. Professor Bridgman's essay differs from most in this respect, for he does offer a suggestion for the construction of a new wing to the edifice, and a direction in which experimenters might seek new phenomena. Throughout the discussion, the distinction between real experiments, conceivable experiments, and pencil-and-paper work is carefully made; the influence of language on our mode of thought is also a prominent theme.

In the first of the four chapters into which the book is divided, the author examines fairly exhaustively the generalized law of the conservation of energy. The questions of the localization of energy and how far a flux of energy is a flux of *anything* are discussed in some detail.

The second chapter deals with entropy and the second law of thermodynamics. Here the modern developments of experimental technique, which enable us to observe statistical fluctuations in such phenomena as Brownian movement and the small-shot effect, have made a re-examination imperative. It no longer seems so outrageous as it once did to imagine a mechanism which should sort out faster molecules from slower ones, and thus (by an inanimate material agency) create a temperature difference where previously there was none. It is possible, of course, that there is something in the nature of atoms and molecules which would prevent this; the signal (presumably of radiation) by which the atom heralds its approach might react on the particle in such a way as to prevent the mechanism from working.

In addition to these two chapters, which occupy about four-fifths of the book, there are a recapitulation and a short but intensely interesting chapter on 'miscellaneous considerations,' including a penetrating analysis of the principle of detailed balancing.

This short account of the book must give a very imperfect idea of the contents, but this is of little importance, for all physicists who are interested in the rationale of what they are doing should read the original.

J. H. AWBERY

SOME PROBLEMS OF DESERT ARCHITECTURE

The Physics of Blown Sand and Desert Dunes, *by R. A. Bagnold. Pp. xx + 265. Methuen & Company Limited, London.* 1941. 24*s. net.*

MR CASAUBON remarked with regret concerning his young cousin, that he had 'no bent towards exploration, or the enlargement of our geognosis.' Such a charge cannot be brought against LIEUT-COLONEL BAGNOLD, whose distinction as a traveller is well known, and who has, in this his latest book, made a notable addition to our knowledge of the physics of the earth's surface. And it is specially pleasant, in these days of physical experiments carried out on an engineering scale, to record one's appreciation of the work of an investigator who can observe closely such impressive natural phenomena as the growth and movement of gigantic desert dunes, and can seek—and find—an explanation of his observations in simple, ingenious and carefully controlled laboratory experiments.

The phenomena which the author records are indeed most impressive in their magnitude and regularity. To those of us whose knowledge of sand dunes is limited by the chaotic and disordered dunes of our own shores, overgrown with vegetation and trampled by men and animals, there is something awe-inspiring in the author's description of 'vast accumulations of sand weighing millions of tons' moving 'inexorably, in regular formation over the surface of a country, growing, retaining their shape, even breeding, in a manner which, by its grotesque imitation of life, is vaguely disturbing to an imaginative mind.'

The problem which the author faces is, in general terms, that of the transport of particles of solid by fluids—a problem which has attracted

much, if unsystematic, attention in various branches of applied science; the author mentions, *inter alia*, the use of air-blast for the carriage of grain and of coal-dust, the drifting of snow, and the carriage of silt by rivers. In most instances the knowledge gained is empirical, and the author's work may be considered as that of a pioneer in a difficult and fascinating region.

The book is divided into three sections. The first section deals with laboratory investigations—in which the wind-tunnel plays a large part—of the mechanism of sand-movement. The author shows clearly that particles of the dimensions conventionally given to sand-grains are not, as is commonly supposed, lifted high in the air and carried over great distances, when sand-storms occur. The movement of the greater portion of the mass of sand in motion is confined to a very small height above ground level, and the sand advances by a process of *saltation*—a series of leaps or bounds, alternating with impacts on the ground surface. The second section deals with the formation of ripples and sand-ridges, and the third section with the growth and movement of dunes and with the description and analysis of two types of dune—the barchan dune and the seif dune. Despite the work of CARUS-WILSON and others, much remains to be learnt concerning singing sands, and all physicists will welcome the inclusion in the third section of a chapter on this intriguing subject.

The author is to be congratulated on an attack on a difficult problem which will serve as a model for future workers in this and allied fields. His book may be unreservedly commended to all who desire to enlarge their geognosis.

ALLAN FERGUSON

MODERN WORK ON GENETICS

New Paths in Genetics, *by J. B. S. Haldane. Pp.* 206. *George Allen and Unwin Limited, London.* 1941. 7*s.* 6*d. net.*

The book contains a series of lectures delivered by one of the leading exponents of genetics, at the University of Groningen, Holland, in March 1940. It is intended to show in its five chapters that genetics occupies a central position in the world of science and that it helps to bridge the gaps between bio-chemistry, embryology, and medicine. PROFESSOR HALDANE has shown some of the lines along which genetics has advanced and along which further progress can be expected. The reader will not fail to be impressed by learning how new methods, many of which are the contribution of Professor Haldane himself, have been, and are being, invented and introduced into genetical research. The book contains a great wealth of interesting and important information, presented often from an entirely new point of view, and one regrets that it is unlikely to be so widely read as it deserves to be, owing to the technical terminology, the intricate statistical analysis, and the discussions on the relationship between complex chemical processes, perusal of which would require great determination on the part of one not an expert in the various fields of science. An introductory chapter with a brief exposé of the fundamental principles of heredity would have been a great help to the reader who is not a geneticist.

The value of new methods in genetics is demonstrated in various ways. It is startling to learn that while the causal analysis of many human abnormalities is at present incomplete, purely statistical methods enable us to say a great deal about their mode of determination. Although the statement that 'in the next thousand years the frequency of recessive diseases will gradually increase in urbanized communities, due to increased in-breeding,' may at first appear dogmatic, it is, nevertheless, an inescapable conclusion from the application of the new statistical methods. It is pointed out that when predictions concern the individual there is always an element of uncertainty, but when genetics deals with millions, such as the constitution of human populations, 'probability becomes certainty and conjecture accurate prediction.'

The book contains occasional references to the implications of genetics in politics. The reader may find that the views expressed are not always impartial; but even when they are somewhat tinged, he will agree that Professor Haldane has made him aware of the danger which lies in the fact that a political system, such as that of present-day Germany, can claim a 'scientific basis' for its justification. Professor Haldane shows that the extreme form of the theory of racial superiority is erroneous and that evidence is still lacking which would prove that racial crossing is necessarily harmful.

It is hoped that the book will help towards a deeper understanding of many important and urgent problems of human genetics. For non-geneticists it may be pointed out that the title of the book should more properly read '*Some* New Paths in Genetics,' because Professor Haldane deals only with those lines in which he is personally interested.

P. C. K.

BIOLOGY BY CINEMA

Cine-biology, *by J. V. Durden, Mary Field, and F. Percy Smith. Pp.* 128, *with numerous illustrations. Penguin Books Limited, Harmondsworth.* 1941. 6*d.*

This little book—one of the popular 'Pelican' series—deserves, and will doubtless achieve, a wide success. Its text is unpretentious but good popular science, dealing with *Amoeba, Paramecium, Hydra*, and other invertebrates in a style likely to hold the interest of the general public. What will appeal to the scientific reader is the wealth of fascinating and beautiful photographs, mostly taken from the well-known *Secrets of Life* films of Gaumont-British Instructional Limited. The patience and skill necessary to obtain such photographs cannot easily be estimated except by the expert, but some idea of the difficulty involved may be gathered from the statement in the book that it took months to discover *Paramecia* in conjugation, all efforts—including those of professional biologists—to bring conjugation about deliberately having completely failed. Other subjects most admirably illustrated are *Stentor, Vorticella, Obelia, Aurelia*, and *Peripatus*, as well as some arachnids, crustaceans and insects. Perhaps the most effective of the photographs are those of *Daphnia*, but each of the illustrations is worth the modest sixpence for which the book is obtainable.

Notes on contributors

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Born in 1890, he was educated at University College, London, where he became Andrews Scholar, Trouton Research Scholar, and Carey Foster Research Prizeman. He went to the Rothamsted Experimental Station in 1913 to explore the possibilities of applying modern physics to soil problems, and after military service during the first world war returned there in 1919, becoming Assistant Director in 1923. He is one of the editors of the *Journal of Agricultural Science*. He has travelled extensively in the United States, South Africa, India, and Europe.

The Rothamsted Experimental Station at Harpenden, Hertfordshire, was founded in 1843 by J. B. Lawes (1816–1900). With J. H. Gilbert (1815–1901), Lawes initiated in Great Britain the scientific investigation of artificial fertilizers and erected the first factory for their manufacture. The scope of the agricultural experiments at Rothamsted steadily widened; laboratories were built, field plots laid out, and careful statistical records kept. By the end of the nineteenth century, this quiet English manor had become one of the principal foci of agricultural research not only in Britain but throughout the world, and in 1909 its status was officially recognized by the Government, which granted it financial support. The present Director is Sir John Russell, D.Sc., F.R.S.

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Was born in London in 1887 and was educated at St. Dunstan's College, the Universities of London, Manchester, and Heidelberg, and the Cavendish Laboratory at Cambridge. Since 1928 he has been Quain Professor of Physics in the University of London. He served as an artillery officer in France in the war of 1914–18, and afterwards was for some years professor at the Artillery College (later the Military College of Science). His interests are wide and varied, ranging from the history of science to art, poetry, and music. He has established a flourishing school of physics at University College, where his laboratory was completely destroyed by bombs in September 1940. Since the beginning of the present war he has been in Government service as Scientific Adviser in the Ministry of Supply.

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Was born in 1897 and educated at Sherborne School and Lincoln College, Oxford. For some years from 1921 he was a schoolmaster, but soon became known to a wide circle of the general public as a successful expositor of science. Though he acknowledges that his purpose in life is 'to know a little about as many subjects as possible,' he is deeply versed in the history of science, particularly Greek alchemy, and was editor of *Ambix* from 1937 to 1939. Since 1940 he has been Acting Curator of the Museum of the History of Science at Oxford.

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Is a leading expert on textile physics, specializing particularly in the structure of proteins and of natural and artificial fibres. Born at Longton, Stoke-on-Trent, in 1898, he was educated at Longton High School and Jesus College, Cambridge, afterwards becoming Assistant to Sir William Bragg at University College, London, and the Royal Institution. In 1928 he was appointed Lecturer in Textile Physics in the University of Leeds, where his brilliant researches earned him a Medal from the University of Lille, the Gold Medal of the Company of Dyers, and the Warner Memorial Medal of the Textile Institute. He finds his principal relaxation in music.

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Is a Yorkshireman, having been born at Sheffield in 1883. He was educated at the Royal Grammar School, Sheffield, and St. John's College, Cambridge, and for a dozen years from 1912 carried out research in the Cavendish Laboratory. From 1921 to 1924 he was Lecturer in Physics Applied to Medical Radiology in the University of Cambridge, a post which he relinquished in the latter year on his appointment to the Chair of Physics in the University of Reading. His contributions to the original literature of his subject have been numerous and important, but—in normal times—he likes occasional moments of leisure for tennis and bridge.

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Was educated at Bury Grammar School and Manchester University, afterwards entering the chemical industry. Here his chemical instinct and organizing ability found full scope, and he was elected Vice-President of the Society of Chemical Industry in 1935. Four years later, he became President of the Society of Dyers and Colourists and a Member of the Council of his old university. He has contributed numerous articles on dyestuffs and related topics to technical and scientific journals and possesses an unrivalled knowledge of the dyestuff, colouring matter, and pigment industry. He is Chairman of the Dyestuffs Group of Imperial Chemical Industries.

RONALD MELVILLE,
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Is a native of Bristol, but soon removed to Cardiff, where he studied pharmacy at the Welsh College of Pharmacy, gaining the M.P.S. and Ph.C. diplomas. Breaking away later from pharmacy he joined the staff of the Imperial College of Science in 1930 and engaged in research on physiology of the tomato. He took up his present post on the Museum staff of the Royal Botanic Gardens, Kew, in 1934, and now deals with questions of economic botany, especially those concerning medicinal plants and the taxonomy and uses of hardy trees and shrubs.

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